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Dr JOHN CARL FLUGEL

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OBITUARY NOTICE

DR JOHN CARL FLUGEL

John Carl Flugel was born in London in 1884 and died there on 17 August 1955. He leaves a wife, daughter (Mrs Hugo Harper) and grandson (Nicholas).

He was successively Demonstrator in Psychology, Senior Lecturer, Assistant Professor, and, after his retirement, Special Lecturer at University College, London, serving the Department with devoted faithfulness until the day of his death. The British Psychological Society owes much to his support as Honorary Secretary, Honorary Librarian, President and, latterly, Chairman of the Social Psychology Section. With Dr C. S. Myers, he was largely responsible for the changes by which the Society, from being a small, select body of professionals, became a large and still growing association of generally interested people. In 1950 he was President of the Psychology Section of the British Association for the Advancement of Science.

He was privately educated until he went up to Oxford, with an exhibition, at the early age of seventeen, to read Classical Moderations and 'Greats'. At once he showed independence by attending Prof. Gotch's lectures on physiology, studying experimental psychology under Dr William McDougall; hypnotism in a practical way, and joining the Society for Psychical Research.

Though many English people could have easily recognized that Flugel had been to Oxford, he had none of the personality signs which, rightly or wrongly, are attributed to the Balliol man of his time. He widened his interests and experience further by studying experimental psychology at Würzburg under Prof. Oswald Külpe. His sympathetic—may one also suggest empathetic?—account of the Würzburgers' 'Denkpsychologie' may owe much to the fact that he understood its attractions and defects from the inside.

An early research, carried out with Cyril Burt, employed intelligence tests in a way unusual at that time, displaying the social interests of these young psychologists by comparing the performances by the boys of a Liverpool elementary school and a well-known Oxford school; many of its pupils sons of intellectually distinguished or socially successful parents.

He was such a prolific writer that even to list his publications, over eighty, would require the space allotted to this article. It may afford some insight into his many-sided nature and his rich culture (he could be informative and witty in five European languages and informative in Esperanto) to mention some outstanding books and to comment on the interests and motives behind their production.

Understandably enough, his first book, *The Psychoanalytical Study of the Family*, reflects Freud's early views more directly—perhaps more obediently—than Flugel's latest books did. There had at that time been little co-operation between psychologists and anthropologists, who had only just began to report facts which seriously complicate any simple picture of loves, hates and tensions within a family. The developing concepts of 'culture-pattern', with the reminder 'Some do and some don't', were hardly above the horizon.

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DAVID HA [illegible] AINING COLLEGE

He was now busy with laboratory experiments and discussed some of his researches in *Practice, Fatigue and Oscillation*, developing concepts for which the University College, London, laboratory became well known. His years of work in conducting and directing research proved that though a convinced and practising psycho-analyst, he was keenly interested in scientific experiment in the strict and narrow sense (at that time) of the term. His example to students, and his enthusiasm for both these branches of study, were of the greatest service to Prof. C. E. Spearman, whose own interests were developing in other directions. Flugel was one of the few British psychologists to be simultaneously interested in laboratory experiment, factor-analysis and psycho-analysis.* While he did much more than his contemporaries to dovetail these three branches of study, it cannot be said that they have yet been really amalgamated.

His generous encouragement of students, especially perhaps of the foreign, lonely and troubled ones, went much further than an interest in laboratory working hours. At his house in Regent's Park, and later in Hampstead, he and his wife, who herself is deeply interested in psychology and wrote several articles for the journals, welcomed hundreds of guests, lecturers and students, and made them feel at home, usually by talking to them in their own language. Any distinguished psychologist who visited London to lecture was sure to be entertained in some way by the Flugels, and to meet people, specially invited, conversant with his special subject.

Either deliberately or for temperamental reasons, Flugel was apt to break, or cheerfully to ignore, taboos. Unlike some contemporaries, he did not defy a convention for fun or to court publicity. More often it was done after serious consideration, and quietly, to save the feelings of as many persons as possible. Dazzling the bourgeois would have given him no thrill; he was one himself, and probably proud of it. Social differences were for him part of the general English scene; he took them for granted. There are plenty of well-trodden paths—perhaps even a few tramlines—in psychology to-day, but Flugel preferred to climb a cliff; usually alone. His *Psychology of Clothes* strides cheerfully over several *Verboten* signs; the subject is hardly mentioned to-day by social psychologists, its discussion being discreetly left to the anthropologists, with their special dispensation. This book is one of the most interesting that he wrote. Though in it speculation is at times difficult to distinguish from fact, the argument cannot fail to interest any thinker unafraid of appearing unacademic.

While he was never an anti-intellectual or an irrationalist in the sinister senses of these terms (indeed, his sturdy and constant support of the South Place Ethical Society, and his many addresses at Conway Hall are cogent evidence to the contrary), he never accepted a neat division between the 'faculties' of Reason and Emotion; a subject about which so many unconvincing books have been written. Yet, Londoner and traveller that he was, he was deeply impressed by the role of symbolism in our world to-day, with its national flags, swastikas, hammers and sickles, animal totems, religious symbols, titles and prestige-garments. At times his suggestions that X might be a symbol of Y provoked attacks from literary people who would rush to defend symbolism in poetry, drama or visual art. It is, however, difficult to counter the common man's objection; 'X may have made Freud think of Y; that was his private affair, not mine', on the level of common sense. But, like Freud, Flugel was fond of speculating—as, indeed, some historians are—about the motives, conscious or unconscious, of famous people long dead, who can offer no relevant evidence.

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* Dr Ernest Jones has described Flugel's relations with and contributions to the psychoanalytic movement in an obituary article in the *International Journal of Psychoanalysis*, 37, part 2. (1956).

A Hundred Years of Psychology has been a classic for years. It is the standby of lecturer and student, to be studied with notebook and pen, yet a joy to read. Erudite, carefully documented, exact, fair, it never displays that frozen attitude often praised as detachment. In each chapter he enters into the spirit of psychologists of the period. To use William James's phrase with a different reference, here was a psychologist incapable of being bored.

In contrast to this selfless, respectful presentation of the thoughts of others, *Man, Morals and Society* gives us Flugel's own views. As an undergraduate, he must have attended many lectures by moral philosophers, but some of these had not yet heard of, or did not wish their circles to be disturbed by intruding psychologists. Yet Flugel felt that the attitude of a single person, however obscure or humble, towards an act which he is about to perform, or to refrain from performing, is important, whatever the totalitarians of administration or theoretical philosophy may hold. As he wrote elsewhere (*Psychological Factors of Peace and War*) in urging more efforts for world integration:

> ...it will be well as often as possible to supplement the cold abstract notion of the state with the warm concrete images of individuals, not, of course, the 'stereotyped' individuals, corresponding to the traditional notion of the Englishman, German, Frenchman, Russian, Japanese, etc, but real men and women in all their rich humanity and variety....The more we are made to think of what war means to individuals, the blasting of their bodies, the blighting of their hopes and the utter disorganization of their plans, the pathetic destruction of the little things they loved and used—all of them matters which we ourselves can understand from our own personal experience—the more will the monstrous moral iniquity of war be made manifest to us.

He differed from many writers about war and peace in that his detestation of war was combined with an extraordinary insight into its present-day causes. The whiffs of opium; 'Nobody wants war' and 'Wars are due to fear', had no effect on him. He calmly listed the social forces which to many people make peace appear dull, and war, especially in prospect, attractive. They make uncomfortable reading to-day.

If it is fair to charge social psychologists with neglecting 'non-psychological' causes of war, Flugel was certainly exempt. He wrote *Population, Psychology and Peace*; on overpopulation as a cause of wars, and in his writings showed very clearly the difficulties in the way of instilling supranational loyalties, not disguising his opinion that few people are trying hard to do this.

In the best sense of the word, he was an academic. If he was chairman of a meeting, it would be conducted as one expects in a seat of learning. He would listen to the speaker with unfeigned interest, and make opening comments invariably related to the paper which had been contributed. Never pedantic, he wore his learning lightly; in the best tradition, he often appeared to be merely reminding the audience of something they knew already, but might temporarily have forgotten.

Why social psychologists so often exclude from their writings the discussion of humour and wit (laughter is more respectable) is a problem far from insoluble. Flugel's long, richly annotated chapter 'Humour and Laughter' in the new *Handbook of Social Psychology* (edited by Gardner Lindzey) reminds us that though humour has a variety of meanings, they are all derived from the original sense of 'moisture, fluid'. And, whatever he wrote, it was never dry.

Though he might have been startled at the appellation, he was a gospeller, on the subjects of friendship, mutual understanding and world peace. A recent broadcast was on Tolerance. Probably the last of his public services was to give an address on 'The implications of psychology in modern social life', an excellent account of what psychologists today are trying, and not trying, to do. The audience was chiefly composed of people from other countries.

He never behaved like an elder statesman expecting youth to show deference, though he punctiliously accorded it to his elders. He was always young in mind, and the photograph represents him as he will be remembered by his older friends.

Bertrand Russell has written 'We must cleanse our hearts of the poisons that make war seem reasonable: pride, fear, greed, envy and contempt.' Jack had done this before I first met him. And that was forty years ago.

T. H. PEAR

METHODOLOGY OF TRAIT ANALYSIS

By WILLIAM STEPHENSON

University of Chicago

It is proposed that traits, as assessed by persons (X's) about their fellows (Y's), are best explained from a self-theoretical standpoint, and not, as hitherto, from a so-called 'objective' framework. Theoretically, modes of regard of the X's (not to be confused with 'halo' effects) are at issue, and not elements of temperament of the Y's. Modes of regard can be represented by factors, attributed to the X's and not to the Y's. Methodological matters are discussed which support this new look at traits and their analysis.

I. The problem

Studies of personality traits by Cattell (1945, 1946*a*, *b*, 1947, 1948), Burt (1946, 1948), Eysenck (1953) and others are pursued in a highly systematic manner, with the best technical resources, so that they appear to be on the right lines scientifically. Clinically orientated psychologists, however, do not find that these studies help them to understand personality any better (Albino, 1953; Uytman, 1954), and it is easy to suggest that the factorists have a great deal of science and many operations, but no theory, whereas the clinical psychologists have things the other way around. The problem, perhaps, is to provide the former with a theory or theories. I propose, therefore, to subject a paper by Cattell, as representative of these factor studies, to a methodological examination, and to provide it with the appropriate kind of theoretical framework. The paper under consideration (Cattell, 1947) is one in which the author analyses thirty-six personality traits which are central to his 'description and measurement of personality' (Cattell, 1946*a*). The data (Reyburn & Raath, 1949) have been shown to be highly consistent (Banks, 1948), although only in some respects (Reyburn & Raath, 1949), and the facts appear to fit well not only with Cattell's own views, but also with those of Eysenck (1953) and of Burt (1946) as a re-factoring by Banks (1948) has indicated. There is little reason to doubt that the facts might fit still other theories. The thirty-six traits have a comprehensive coverage of the literature, and it has somehow seemed important to study their kind, to judge by the attention given to them by Thurstone (1947*b*), Guilford & Guilford (1939) and others.

II. Methodological considerations

The concern in Cattell's study was with students (X's), who assess their fellows (Y's) for the temperamental traits of the latter. The traits were of the kind *cheerful, energetic, sociable.* The conditions under which the ratings were undertaken were carefully prescribed, as follows:

> The proper planning of rating...requires such essentials as that the raters live with the ratees; have no official relation to them; are not asked to rate more than about twenty people, because more cannot be known well; deal with all subjects at one time with respect to one trait and not all traits with respect to one subject; have traits behaviourally defined and illustrated; have the ratings pooled for at least ten judges to cancel individual perspectives (Cattell, 1947, p. 198).

Under such conditions, Cattell proposes, the 'only remaining source of systematic error is halo or stereotype', which, he adds, has never been demonstrated in trained raters. Data gathered under these conditions were correlated and factored. The factors are thereupon referred to the Y's, the subjects under observation, by the X's. This, I propose, is an operational mistake. The operations are not those of the Y-population, but of the X's. One should ask, therefore, what might be expected when relatively naïve subjects, such as the X's, observe their fellows in terms of designated traits? It is possible that everything might be explained, theoretically, from the frame of reference of the X's. That is, there might be a theory which is intrinsic to the X's, and this apart from halo effects, or from any arbitrariness on the part of X's. The concern might be less with the supposed personalities of the Y's and more with the way we acquire and use traits; it may be wondered, for example, how naïve subjects, such as the X's, are able to give meaning to complex traits such as *punctilious* or *hypochrondriacal* or *unscrupulous*. Little seems to be known about the development of language in relation to human behaviour, and a theory for such matters would perhaps be of some general interest.

A second gap in Cattell's study stems from two related sources, first from the lack of any deductive possibilities for the factors he reaches, and secondly, from restrictions imposed upon his data arbitrarily by the *simple structure* solution to the rotation of his centroid factors. As for the first: Cattell distinguishes between *principal surface* and *principal source* (or primary) traits, the latter being elements of which the former are composed. Given a surface trait it is always possible to analyse it into its constituent primary or source traits. But, given the primaries, Cattell does not deduce from them what surface traits are possible. The situation is like that for flagstones; given a pavement one can always distinguish the primary flagstones, but given the flagstones, what can one make with them? Clearly a great deal, and much besides flagstones. Cattell's system works only one way, down into primaries, but not up again into complexes.

As for *simple structure*: this, as is well known, looks for a solution to the rotations, such that no variate is on more than one factor. In Cattell's study twelve centroid factors are rotated to reach twelve primaries (A to L) in simple structure. There are no traits, if the solution is reached, compounded of two or more primaries; certainly such composites are minimal, for it has been usual in the past to reject tests for which primary factors cannot be reached, on the ground that they are impure mixes of other, well-established, primaries. French (1949) has noted that tests of scholastic attainment are impure in this way. However, it is scarcely possible to discard *traits* from a matrix because they are impure mixes of primaries. Equally, it is scarcely adequate to regard complex traits, such as Cattell's principal surface traits, as mere mixes of primaries, in the sense that so much white and so much black make grey. The essential problem is to *explain* the complex traits in terms of some primaries. If this could be done, then a remedy would be at hand for the weakness just noted, that Cattell's system works down into primaries but not up again into complexes. Before developing such an explanatory system, however, it is worth noting that Cattell's search for simple structure may have resulted in an analysis into too many unrelated, and *unrelatable*, primaries. There is a possibility that data might be analyzed too far, into fragments rather than into properly analytical primaries. Banks (1948), it will be recalled, required only four or five of Cattell's twelve centroid factors in order to explain Cattell's thirty-six traits to her satisfaction. The other centroids could be regarded as either insignificant, or as minor and unimportant complications of the main factors. It is possible, likewise, that four primaries, *plus the composites they generate*, may have advantages for explanation which are lost when the same matrix of variates is reduced to twelve primaries in simple structure. In short, *simplest* structure (as I have elsewhere called it (Stephenson, 1953)) may be the key to the analysis of Cattell's traits, rather than analysis into too many factors in *simple* structure.

III. EXPLICIT AND REDUCTION DEFINITIONS

The problem is to explain complex traits in terms of relatively few primaries. Clearly, much depends upon what is to be meant by an explanation. Factorists have rarely hesitated to offer explanations, as interpretations, of the factors they discover, but are often criticized for hindsight. The issues, however, have never been properly examined from a logical or philosophy-of-science standpoint. Albino (1953) has had something to say in this direction, but there is much in the work of Peirce (1950), Carnap (1936) and others which has application to factor methods, and which has long been overlooked.

For the present, following Carnap, it is perhaps reasonable to proceed as follows. Any interpretation of a factor is a definition. But one should distinguish between explicit definitions and convergent reduction chain definitions. When Thurstone finds a primary factor for a *numbers* test, to call it a primary ability is merely to give the test an explicit definition. Nothing about the *numbers* test is explained in any hypothetical way. The situation can be very different for a reduction definition. To put salt in water (to use the familiar example) is a test condition (*Tc*); to see it dissolve is the test result (*Tr*). To say that the salt is *soluble*, however, is to attribute to the salt-in-water an attribute or characteristic (*Qs*) with the status of an *ad hoc* explanation. We say, likewise, that lead is *insoluble* in water, and this has the same *ad hoc* status. But solubility might be a fruitful term, and insolubility not; one may wonder whether colloidal, amorphous mixes, or other physical or chemical properties, are involved when salt dissolves in water, but there seems to be nothing to explain about the lead-in-water. Interest in science is an *ad hoc* explanation where the conditions provoke curiosity, and where there are immediate possibilities for hypotheses about the *Qs* term. Thus, if it is suspected that salt-in-water is an amorphous mix and not a colloid, the matter would have to be discussed theoretically, deductions would be drawn, and methods sought for putting these to empirical test.

I believe that it is true to say that few provoking reductions have been found in factor studies of abilities, whereas they are apt to run riot in studies of personality. Thus, if tests *A* and *B* are (respectively) tests of *numbers* and *colour dominance*, and define one and the same primary factor *f*, the conditions exist for curiosity. The test result is that *A* and *B* are testing the same thing; and explanation is required, then, which apparently has to transcend the explicit definitions of the two tests—neither numbers, nor colour dominance can explain the factor. Thus when Albino (1953) argues that the definition of tests by *simple structure* is tautologous, a matter of explicit definitions only, the trouble is not with simple structure as such, but with the fact that startling reductions do not, apparently, occur for tests of ability. The analysis is into fragments, and not into provocative reduction terms. The situation is logically far more promising for second-order factors, since each embraces a number of different primaries; but few factorists give these much concern, on the grounds, apparently, that the basic explanations lie with the primaries. In any case, second-order factor reductions may or may not be fruitful—the situation may be like that for lead-in-water rather than for salt-in-water. Matters are more provocative in gathered under these conditions were correlated and factored. The factors are thereupon referred to the Y's, the subjects under observation, by the X's. This, I propose, is an operational mistake. The operations are not those of the Y-population, but of the X's. One should ask, therefore, what might be expected when relatively naïve subjects, such as the X's, observe their fellows in terms of designated traits? It is possible that everything might be explained, theoretically, from the frame of reference of the X's. That is, there might be a theory which is intrinsic to the X's, and this apart from halo effects, or from any arbitrariness on the part of X's. The concern might be less with the supposed personalities of the Y's and more with the way we acquire and use traits; it may be wondered, for example, how naïve subjects, such as the X's, are able to give meaning to complex traits such as *punctilious* or *hypochrondriacal* or *unscrupulous*. Little seems to be known about the development of language in relation to human behaviour, and a theory for such matters would perhaps be of some general interest.

A second gap in Cattell's study stems from two related sources, first from the lack of any deductive possibilities for the factors he reaches, and secondly, from restrictions imposed upon his data arbitrarily by the *simple structure* solution to the rotation of his centroid factors. As for the first: Cattell distinguishes between *principal surface* and *principal source* (or primary) traits, the latter being elements of which the former are composed. Given a surface trait it is always possible to analyse it into its constituent primary or source traits. But, given the primaries, Cattell does not deduce from them what surface traits are possible. The situation is like that for flagstones; given a pavement one can always distinguish the primary flagstones, but given the flagstones, what can one make with them? Clearly a great deal, and much besides flagstones. Cattell's system works only one way, down into primaries, but not up again into complexes.

As for *simple structure*: this, as is well known, looks for a solution to the rotations, such that no variate is on more than one factor. In Cattell's study twelve centroid factors are rotated to reach twelve primaries (A to L) in simple structure. There are no traits, if the solution is reached, compounded of two or more primaries; certainly such composites are minimal, for it has been usual in the past to reject tests for which primary factors cannot be reached, on the ground that they are impure mixes of other, well-established, primaries. French (1949) has noted that tests of scholastic attainment are impure in this way. However, it is scarcely possible to discard *traits* from a matrix because they are impure mixes of primaries. Equally, it is scarcely adequate to regard complex traits, such as Cattell's principal surface traits, as mere mixes of primaries, in the sense that so much white and so much black make grey. The essential problem is to *explain* the complex traits in terms of some primaries. If this could be done, then a remedy would be at hand for the weakness just noted, that Cattell's system works down into primaries but not up again into complexes. Before developing such an explanatory system, however, it is worth noting that Cattell's search for simple structure may have resulted in an analysis into too many unrelated, and *unrelatable*, primaries. There is a possibility that data might be analyzed too far, into fragments rather than into properly analytical primaries. Banks (1948), it will be recalled, required only four or five of Cattell's twelve centroid factors in order to explain Cattell's thirty-six traits to her satisfaction. The other centroids could be regarded as either insignificant, or as minor and unimportant complications of the main factors. It is possible, likewise, that four primaries, *plus the composites they generate*, may have advantages for explanation which are lost when the same matrix of variates is reduced to twelve primaries in simple structure. In short, *simplest* structure (as I have elsewhere called it (Stephenson, 1953)) may be the key to the analysis of Cattell's traits, rather than analysis into too many factors in *simple* structure.

III. EXPLICIT AND REDUCTION DEFINITIONS

The problem is to explain complex traits in terms of relatively few primaries. Clearly, much depends upon what is to be meant by an explanation. Factorists have rarely hesitated to offer explanations, as interpretations, of the factors they discover, but are often criticized for hindsight. The issues, however, have never been properly examined from a logical or philosophy-of-science standpoint. Albino (1953) has had something to say in this direction, but there is much in the work of Peirce (1950), Carnap (1936) and others which has application to factor methods, and which has long been overlooked.

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Cattell, in fact, interprets several factors by reduction terms. His factor H (1946*a*, p. 328) is called *adventurous cyclothymic* versus *withdrawn schizothymic*. It embraces such traits as *kindly*, *idealistic*, *open*, *sentimental*, *lusty*, *trustful*, *expressive* and the like on the one side, and on the other *cynical*, *schizoid*, *spiteful*, *aloof*, *misanthropic*, *timid*, *paranoid*, *narrow*, etc. That these traits cluster in the factor space is a test result; and in so far as they have different meanings and are not merely synonymous in the dictionary sense, they require a reduction explanation. One has to ask, therefore, whether *cyclothymic* versus *schizothymic* is a provocative reduction term. It is clearly not, if it does not lend itself immediately to hypothetical treatment leading to testable propositions. Cattell offers no such treatment. His principle surface traits (Cattell, 1946*a*) are analyzed into primaries, much as brown can be analysed into the three primary colours. Complicated forms of behaviour are thus presumed to be composed of various combinations of twelve or so primaries. There, somehow, the matter rests; but this is not because one's curiosity has been assuaged—rather, it has perhaps not been stimulated.

IV. A THEORY OF VALUE-SYSTEM MODES OF REGARD

The material situation concerns X's who observe others, the Y's, in relation to trait names. I am to suppose that the X's have a few basic modes of regard, of a value-system order, and that these explain complex traits.

The theory follows the lines of self-psychology, such as stems from Cooley (1909) or Mead (1934). The X's are no longer regarded as 'objective' sounding boards, reflecting 'objectively' what they see of the Y's. Rather, behaviour of a self-referent kind is at issue, such as one finds in the *me-you* dynamisms of Sullivan (1940)—the husband, it will be remembered, who thinks of himself as *amiable*, thinks of his wife, correspondingly, as *devoted* to him. Thus, in trait analysis one has to find what *me*'s, so to speak, go with what *you*'s in the dynamic situation linking the X's and the Y's. The X's know the Y's very well, as Cattell also supposes; I would perhaps like to add that they know their *kind* very well. I assume that the X's assess the Y's within the framework of a value system, for the main part. They regard the Y's as *unfair*, *naughty*, *goody-goody* or the like, as though moral standards are at issue. One might *look*, therefore, for factors with such explanations. The Y's might also notice the very obvious fact that some Y's run about more than others, and thus seem more *active*; only very obvious 'objective' facts of this kind are likely to be observed spontaneously by X's.

The theory, then, proceeds as follows: First, traits are considered to be meanings, and not general propositions or actuarial assumptions about behaviour. The point has been made before by Kantor (1933) and myself (Stephenson, 1953). Secondly, it is proposed that the X's assess the Y's by way of a system of *modes* of regard, there being relatively few basic modes, of a simple-minded or very obvious kind. Thirdly, the system is such that complex traits may be supposed to develop out of these simple ones in a coadunating fashion, which, in its simplest form, may be compared to a mechanical relay system such as Tustin (1953) and others have described for analogous brain conditions.

With respect to modes of regard: these are reached concretely in Q-technique studies, much as Freidenberg & Roth (1954) indicate for the conceptions of university students about their campus milieu, or as I have discussed for Q-sort descriptions of personality (Stephenson, 1953). Factors, for such Q-sort descriptions, represent modes of regarding others in relation to oneself. They are learnt ostensibly, in a social milieu, and may be compared to types or models—what we think the typical Frenchman is like, or the typical milkman, or the typical English schoolboy, are examples of the kind of formations here postulated. They are total, global affairs, reflecting the motivations and prejudices of the X's. They are likely to involve moral evaluations in Cattell's study; the concern will be with the typical *naughty* student, or the 'dumb blonde' one, or the like. They are total in the sense that it takes not less than a Q-sort, for a large sample of traits, to describe them adequately. A verbal tag may be given to these modes, e.g. 'dumb blonde'. They are clearly not ultimate in any sense; nor need they be fixed things—they may change, as Hebb (1949) suggests for trait names, and yet retain their verbal tag. Here it is supposed, however, that some modes are rooted in earlier development than others. The modes correspond roughly to Mead's 'generalized others'.

The third prop of the theory is that complex traits are compounded from or linked with a few such basic modes of regard. The few may be regarded as keys in a mechanical relay system, leading to networks which are reached by pressing this or that combination of the keys; in this manner one proceeds, neurologically, from the schema concerned with basic modes to those for more complex traits. The model has been employed often before, for example, by Tustin (1953) and McCulloch (1944).

The material system, then, is conceived as meanings that X's have of 'generalized others'; some of Cattell's traits refer to these as verbal tags. Along Q-technique lines these 'others' can be demonstrated facts; for Cattell's data, however, they have to be inferred. Combinations of these occur in a relay system fashion, which at least does not contradict what would be called, nowadays, sensible 'brain talk' (Young, 1951).

V. THE FORMAL MODEL

The formal part of the theory, from which the deductive and testing possibilities stem, and which covers the material system isomorphically, is a system of factors, as follows. Let there be a small number of orthogonal factors, say A, B, C and D. These can be combined none, one, or more factors at a time. For positive and negative loadings (such as occur for bipolar factors), the arrangements are as follows:

Null-conditions: none of the factors.
Single conditions: A, B, C, D and correspondingly, $-A$, $-B$, $-C$, $-D$.
Two factors: AB, AC, AD, BC, BD, CD, each with four possible arrangements of the signs, e.g. AB, $(-A)B$, $A(-B)$, $(-A)(-B)$.
Three factors: ABC, ABD, ACD, BCD, each with eight possible arrangements, e.g. $(-A)BC$, $A(-B)C$, etc.
Four factors: $ABCD$, with sixteen possible arrangements of the signs, e.g. $(-A)BCD$, $(-A)(-B)CD, \ldots$, etc.

The rules for making deductions are as follows. For bipolar traits, given any combination of the factors, the poles consist of the same factors with signs reversed. Thus, $A(-B)(-C)$ is the opposite pole of a trait whose factor composition is $(-A)(B)(C)$. Another deductive possibility is that certain combinations of factors are mutually incompatible, and therefore do not occur empirically. These deductions are carried out within a
system which may or may not correspond to dictionary definitions of traits, but all lead to
testable propositions. As just stated, $A(-B)(-C)$ is the opposite pole of a trait whose
factor composition is $(-A)(B)(C)$. If *hypochrondriacal* has the latter factors, a trait with
the diametrically opposite meaning will have the former. Dictionaries have been unable,
in the past, to say what is the opposite of *hypochrondriacal*, except to say that it is *not-*
hypochrondriacal—which of course could cover almost anything in the world. We may be
able to deduce, however, given the primaries A, B, C, that *punctilious* should have a factor
composition diametrically opposite of that for *hypochrondriacal*. It is then a simple matter
to put the deduction to empirical test. If the trait *punctilious* does not appear in Cattell's
list of traits, one would include it in a new study of traits, to see whether the predicted
factor composition occurs for *punctilious*. Again, if it is deduced that no trait can occur
with the factor composition CD, the fact that this combination has not occurred pre-
viously in factor studies, or that it does not occur in studies designed to test it, is
evidence that the deduction is on the right lines. The theory as a whole, then, will be the
more credible the greater the number of such deductions one has put to empirical
proof.

The idea of a network in the material system, clearly, is here modelled by factors, where
the factors correspond to basic modes of regard, and compounds of these to complex
traits.

VI. RE-ROTATION OF CATTELL'S CENTROID FACTORS

It will be recalled that Cattell or his assistants spent the equivalent of one man-year of calculations for his solution to the multiple-factor problem for the twelve centroid factors extracted from the data for the thirty-six traits (Cattell, 1947), and much longer than this for other comparable data. Banks (1948) refactored the latter table, spending a month's work upon the solution. My own solution, with a theory to guide the rotations, takes less than two hours for either of Cattell's tables. It proceeds as follows, using Table 1 of Cattell's 1947 paper for the present explication.

A solution is required which will satisfy these conditions: orthogonality will be retained, in keeping with the requirements of the formal factor model; *simplest structure* (Stephenson, 1953) will be looked for, and not *simple* in Thurstone's sense (Thurstone, 1947*a*). A third condition for an acceptable rotation concerns the rules for making the dependent factor analysis; here one argues, from the self-theoretical framework of the X's, that such traits as *jealous* and *suspicious* are likely to hang together about a primitive mode of regard. One would expect, on the same commonsense grounds, that *easily upset* and *anxious* (or *worrying*) would also so hang together, and likewise *alertness* and *energetic*. Each pair, indeed, could almost suggest a little dynamic system for itself, in which the terms relate as causes and effects. For the moment, however, it is expected that *jealous* and *suspicious* will have reference, for naïve X's, to students who are of a somewhat depraved kind, the 'naughty' people, so to speak, of the campus. Similar considerations apply to *anxious* and *easily upset*; these must surely be key verbal tags for a stereotypical regard of certain 'others' who are readily recognized by X's. The same applies for the *alert, quick, energetic, lively* personality; what X's are unlikely to have a stereotype of such a high-tempered clavicord? Now I do not know precisely which of such modes will occur empirically for Cattell's data; but the facts can be looked at with expectancies of the above kind in mind (a scientific procedure about which I have written elsewhere as an example of the logic of abduction, a method of inference originally outlined by the American philosopher C. S. Peirce (1950) and discounted ever since), and the rotations of Cattell's centroid factors made in accordance with such expectancies. That is, the necessary factors are reached by *observing* that certain variates hang together as a factor, as *expected*. Another deduction involves variate no. 36 (the intelligence of the Y's as objectively measured): one assumes that it must tend to be orthogonal to most personality traits, and, therefore, no solution to the rotations will be acceptible which does not have no. 36 distinctly on a factor which is orthogonal to all other factors, and which does not 'make sense' in relation to other traits.

With these conditions in mind, then, one proceeds with the rotations. It is scarcely necessary to outline the steps taken to reach a solution, except to say the following. As Banks (1948) notes, the bipolarities for Cattell's traits do not all run in the same direction, resulting in many negative correlations which confuse matters somewhat unnecessarily. Reversals were therefore made, it being sufficient to do so for Cattell's traits nos. 1, 2, 5, 7, 9, 12, 13, 15, 16, 18 and 27. Using the graphical method (Zimmermann, 1946) for the centroids two at a time, the solution is very simply reached by using trait no. 11 (*suspicious*) to define factor A, and therefore also B (since the factors are orthogonal), and no. 26 (*anxious, worrying*) to define C and D. That is, Cattell's first four centroid factors are rotated to A, B, C and D respectively, using these few traits as guide-posts for the dependency reasons to which allusion has already been made. It is then observed that Cattell's sixth centroid relates to D, and by way of traits nos. 8 and 26 as the defining traits, a factor E is assumed. Cattell's seventh centroid, likewise, relates to C, and allows us to place trait no. 36 on a factor F, C being swung round to be orthogonal to F. The remaining centroids (nos. 5, 8, 9, 10, 11 and 12) offer no scope for dependency rotating, and are therefore discarded. It will be noted that Banks also found that she could dispense with many of Cattell's centroid factors, and that Thomson (1947) had earlier reached the same conclusion. We end, then, with six orthogonal factors A–F, five of which are well enough defined. The solution is given in Table 1. If loadings of 0·30 or more are considered to be significant, the typification at the right of Table 1 covers the data. Traits 11, 22 and 24 define A; traits 9, 16, 25, 32 and 34 define B; traits 17, 20, 21 and 35, define C; traits 8 and 26 factor *D*. The intelligence test focuses factor *F* for us, and *E* appears only in combination with other factors. The situation is one of *simplest* structure in our sense (Stephenson, 1953).

Table 1. *Cattell factors rotated to simplest structure*

(Involving the first seven factors of Cattell's unrotated, centroid factor matrix (Cattell, 1947, p. 207).)

Variables	*A*	*B*	*C*	*D*	*E*	*F*	Typification
1	51	-25	24	-38	-22	-05	$A(-D)$
2	42	10	66	-01	-10	-24	AC
3	54	65	18	16	06	-20	AB
4	41	60	-35	-25	-03	01	$AB(-C)$
5	-31	59	24	00	14	-09	$(-A)B$
6	52	22	33	15	03	-32	AC
7	-32	53	-09	41	15	09	$(-A)BD$
8	21	-13	24	68	01	-14	D
9	-29	70	-14	-12	-02	08	B
10	21	-17	55	16	-36	-44	$C(-E)(-F)$
11	74	-04	03	11	06	01	A
12	65	-07	03	-35	-21	-04	$A(-D)$
13	33	43	43	03	14	-04	ABC
14	38	-02	29	23	35	-01	AE
15	58	37	31	-26	-20	01	ABC
16	10	80	20	00	-11	-10	B
17	17	-11	78	05	-13	-07	C
18	-11	74	40	-16	-12	03	BC
19	44	07	-34	-38	-16	19	$A(-C)(-D)$
20	04	19	81	-08	06	16	C
21	11	-17	53	-23	24	-16	C
22	76	17	06	16	14	17	A
23	64	-32	-27	-01	09	13	$A(-B)$
24	74	26	16	-10	-00	-13	A
25	25	50	-05	-08	20	-03	B
26	26	-04	21	47	-01	00	B
27	36	31	51	-26	-41	07	$ABC(-E)$
28	05	77	-19	-16	31	-03	BE
29	13	25	-04	31	45	08	DE
30	51	-01	-07	36	37	-09	ADE
31	00	34	-27	24	18	16	$B(-C)D$
32	00	46	20	07	06	-06	B
33	18	62	-08	04	13	03	B
34	03	74	26	07	05	-20	B
35	15	-20	68	26	13	-09	C
36	-09	04	01	16	12	57	F

(Decimal points omitted.)

VII. THE REDUCTIONS

The factors are now interpreted. Factor *A* involves the traits *suspicious, jealous, demanding* (with the bipolar counterparts *free from suspicion and jealousy*, and *emotionally mature*, or *thinks of others*). I propose that the generalized factor hypothetically at issue in this case is concerned with a 'naughty' versus a 'gracious' young woman; a 'nasty bit of work' type of woman, perhaps, as contrasted with one of poise and good breeding. Factor *B* clearly requires 'vital' versus 'quiescent' as the reduction term. On the positive side of the factor are the traits *energetic, talkative, unconventional, eccentric, frank, expressive, gregarious, sociable*, with *marked interest in opposite sex*. On the negative side, contrasting with this, are the traits *languid, slow, silent, introspective, secretive, reserved, self-contained* (*goes own way*), and of course with *slight interest in the opposite sex*. The 'socialite' is perhaps the type on the one side, and the 'misanthrope' or the like type on the other side. Factor C is reduced to an 'immature, somewhat crude' person versus an 'independent, somewhat cultured one'; the traits are *fickle, indolent, awkward, immature, dependent* versus *persevering, tidy, polished, independent-minded, charming*. Factor D suggests the 'easily upsettable' person versus the 'tough' one. The traits are *easily upset, anxious, worrying*, versus *unshakable poise, calm, tough*; the 'neurotic' type versus the 'tough guy' comes at once to mind as the generalized others. Finally, for E there are no simply defining traits, but nos. 29, 10 and 27 are involved, and from these I would offer the following reduction term: 'impractical, sensitive' versus 'logical'—the 'day-dreamer' type, as compared with the 'thinker'. Factor F is the objective measure of intelligence.

It is to be remembered that the traits were defined, in the original study by examples and a brief paragraph of description. One must refer to Cattell's paper for a clear notion of what the various traits may be supposed to have meant to the X's. By the same token it is impossible to find any precise verbal tag for the factors, that is, any sufficiently comprehensive word to cover what the reductions involve. All that is required is that the reduction terms point to concrete modes of regard of the X's, that is, to generalized others. The term 'naughty', that is, indicates that the X's carry about with them a concept of certain others in their milieu who are not nice, a little depraved, and the like. The concern is to see these as naïve but concrete matters, such as young students might well have in mind about their peers.

VIII. COMPOUND TRAITS

Whereas Cattell, with his twelve primaries, covered the thirty-six traits more or less precisely by *simple* structure, there being no trait with two or more factors, the structure for our six centroids, rotated, embraces only about one-half in the *simple* condition, leaving the other half to be involved with two or more of the primaries A–F. Compound traits are defined as those having two or more of the factors A–F. It is proposed that compound traits receive an explanation, in a hypothetical sense, in terms of the basic factors.

Consider a few examples. We see from Table 1 that *hypochrondriacal* is compound (AC): it is made up of 'naughty' *plus* 'immature'. *Obstructive* (no. 1) has factors A and $-D$, and thus involves 'naughty' *plus* 'tough'. The trait *somewhat unscrupulous* (no. 27) has factors $ABC(-E)$, and has the meaning, therefore, in terms of the primaries, of 'naughty' *plus* 'vital' *plus* 'crude' *plus* 'logical'. If the reader will muse about these terms for a moment I think that it will occur to him that *somewhat unscrupulous* could well have the character of being somewhat depraved ('naughty') but full-blooded ('vital'), and yet also unpolished and lacking in charm ('crude') as well as being scheming and calculating in the way of a 'practical, logical' mind. Again, trait no. 19, for *stern, hard*, has the factor composition $A(-C)(-D)$, meaning 'naughty' *plus* 'independent' *plus* 'tough'. The opposite pole, for *kindly, softhearted*, is reduced to 'gracious' *plus* 'immature' *plus* 'upsettable'.

The reader must determine for himself how far the composites 'make sense' for the various compound traits of the Cattell data. Meanings of words are notoriously slippery, but there is surely more than fortuitousness in the reductions here under consideration. It is amusing, at least, to see how closely the composites 'hit' the meanings of complex traits, and this, I venture to say, is without exception in the data. The *boor*, for example, is amusingly indicated as $C(-E)(-F)$, that is, 'crude' *plus* 'logical' *plus* 'somewhat *unintelligent*'—showing that *intelligence* as objectively measured, can enter the present scheme of things, contrary to what Banks supposed. But perhaps the most interesting trait in the literature on personality is no. 13, for *emotional* versus *calm, phlegmatic*, that is, concerning the way some people become angry, fearful, jolly, sexual, sorrowful, or express disgust and the like at the slightest provocation; who are frequently emotional and passionately excited; who show marked signs of emotion, even if it is under some control. This trait has factors *ABC* here, yet all factorists up to now have concluded that it is of a fundamental and primary nature. For Burt it is the general factor of personality, and Spearman held a similar belief, except that he believed that *lack of control* was more essentially at issue (his *w*-factor); but Thurstone, Cattell and Eysenck, amongst others, accept the primary nature of such a factor. Here it is grasped as 'naughty' *plus* 'vital' *plus* 'crude' or 'immature', and these seem to characterize the matter very well. The psychologist has assumed previously that conditions of emotion, of sex, jollity, sorrow and the like *should* be basic to personality, and therefore has acquiesced in the notion of a general, or also of a primary, factor of *emotionality*; he did so without proof. It no doubt seems a reasonable interpretation to make. Yet I fail to see the need for it—on the contrary, the key word *emotional* stands, for the *X*'s, for their conception of a person who is somewhat depraved, yet vital, and of course immature and crude; indeed, perhaps the concept *emotional* is derived from such basic conceptions. The *reasons* for any emotionality of the *Y*'s are quite another matter; for the *R*-factorists the reasons *are* the emotionality as such, as a primary fund of something inside the *Y*'s, with a physiological origin and roots. This, however, is perhaps about as far from an adequate reason as anything could be. Spearman, in postulating a factor of self and control (*w*-factor), was on better lines. The emotional quality of a person is a matter for concrete study in the concrete case; to regard behaviour as in some way explained by general emotionality, or by primary emotion, is not a genuine hypothesis; it does not deal with the facts as they can be observed, and it serves (as such theories are apt to do) to stop the inquiring scientist from discovering these facts for himself.

Be this as it may, I provide the complete list of the compound traits for Cattell's data, with their reduction to the basic factors. The data are in Table 2.

It should be remembered, perhaps, that the 'cut-off' of acceptable statistical significance for factor loadings, assumed to be 0·30 above, is somewhat arbitrary. The standard error of these loadings is difficult to assess (Thurstone, 1947*a*). I would draw attention, however, to the surprising way in which the above interpretations apply if the 'cut-off' is reduced. For this reason I have retained above a typification of trait no. 31 (*aesthetically fastidious*) with factors *B*, $-C$, *D* for a 'cut-off' at 0·20 loading; fastidiousness is grasped as 'vital' *plus* 'independent' *plus* 'upsettable', which together seem to make sense for the trait.

Certain of the combinations appear to be impossible empirically; thus, one can scarcely be 'vital' and 'quiescent' at the same time, and in the above system it is predicted that $A(-C)$, $B(-D)$, $AB(-D)$ will never occur in fact. They do not appear in Table 1.

IX. DISCUSSION

It is not supposed that only five or six basic modes of regard occur empirically, or that I have really hit upon adequate modes from Cattell's data, or that any mode is other than a condition of flux for the developing *X*'s. What is interesting, however, is the

Table 2. *Reduction table for compound traits*

Factor compositions	Variables	Compound traits			Corresponding reductions		
		(Positive)		(Negative)	(Positive)		(Negative)
AB	3	Attention-getting (shows off, boastful)	*v.*	Self-sufficient, under no wish to get attention	'naughty' + 'vital'	*v.*	'gracious' + 'quiescent'
$(-A)B$	5	Cheerful	*v.*	Depressed, solemn (unhappy, dour)	'gracious' + 'vital'	*v.*	'naughty' + 'quiescent'
AC	2 6	Changeable Frivolous	*v.* *v.*	Emotionally stable Responsible	'naughty' + 'crude'	*v.*	'gracious' + 'independent'
AE	14	Hypochrondriacal	*v.*	Not so	'naughty' + 'impractical'	*v.*	'gracious' + 'logical-minded'
$A(-D)$	1 12	Obstructive Spiteful, grasping	*v.* *v.*	Ready to co-operate Good-natured	'naughty' + 'tough'	*v.*	'gracious' + 'sensitive'
BE	28	Composed	*v.*	Shy, bashful	'vital' + 'practical'	*v.*	'quiescent' + 'impractical' (sensitive)
BC	18	Adventurous, bold	*v.*	Cautious, retiring	'vital' + 'crude'	*v.*	'quiescent' + 'cultured'
$A(-B)$	23	Rigid, conservative	*v.*	Adaptable, open to compromises	'naughty' + 'quiescent'	*v.*	'gracious' + 'vital'
DE	29	Sensitively imaginative	*v.*	Practical, logical	'upsettable' + 'quiescent'	*v.*	'tough' + 'logical'
ABC	13 15	Emotional Self-willed, egotistical	*v.* *v.*	Phlegmatic Mild, self-effacing	'naughty' + 'vital' + 'crude'	*v.*	'gracious' + 'quiescent' + 'cultured'
$AB(-C)$	4	Assertive, self-assured	*v.*	Submissive, humble, embarrassable	'naughty' + 'vital' + 'cultured'	*v.*	'gracious' + 'quiescent' + 'crude'
$(-A)BD$	7	Attentive to people	*v.*	Cool, aloof, coldly indifferent	'gracious' + 'vital' + 'sensitive'	*v.*	'naughty' + 'quiescent' + 'tough'
ADE	30	Neurotic fatigue	*v.*	Not so	'naughty' + 'upsettable' + 'sensitive'	*v.*	'gracious' + 'tough' + 'logical'
$A(-C)(-D)$	19	Stern, objective	*v.*	Kindly, soft-hearted	'naughty' + 'cultured' + 'tough'	*v.*	'gracious' + 'crude' + 'sensitive'
$B(-C)D$	31	Aesthetically fastidious	*v.*	Lacking in artistic feeling	'vital' + 'cultured' + 'sensitive'	*v.*	'quiescent' + 'crude' + 'tough'
$ABC(-E)$	27	Somewhat unscrupulous	*v.*	Conscientious	'naughty' + 'vital' + 'crude' + 'impractical'	*v.*	'gracious' + 'quiescent' + 'cultured' + 'practical' (logical)

theoretical possibility that along such lines one could account for all the personality affections in Roget's *Thesaurus*. Ten basic factors, combining not more than four at a time, could account for over sixteen hundred traits of affection and their synonyms, and the number becomes astronomical for larger compoundings and a few more basic modes. Again, a basis exists for determining empirically what might be the appropriate synonyms for unusual traits, such as *frippery*, *serene*, *indecisive* and the like.

The differences between the above model and the group-factor solution adopted by Banks can help us to say in what sense the one is a scientific system and the other not. Banks accounts for Cattell's data by a general factor (e) and three bipolars, x, y, z. There are thus seven defining points in the factor model, e, x, $-x$, y, $-y$, z, $-z$. She draws attention to other traits which lie at the eight interstices between the three orthogonal bipolars, namely, at xyz, $(-x)yz$, $x(-y)z$, $xy(-z)$, $(-x)(-y)z$, $(-x)y(-z)$, $x(-y)(-z)$, $(-x)(-y)(-z)$, to which she gives the names paranoid, adolescent, hysteria, compulsive, anxiety, dominating, impulsive, cordial and facile (Banks, 1948, pp. 216–17). One wonders why traits are missing compounded of two of the factors, or of four, or why the *null* situation is omitted. Personality is merely diagrammatically represented as a 14-point star, held at the end of a general factor stick e. Surely there can be no such material thing, either in personality as such of anyone, or in 'brain talk'. No isomorphism can exist between the model and anything material.

Our own model, on the other hand, attempts to deal with material things, to which it has an obvious isomorphism. Everything concerns the internal frame of the X's, who may be supposed to have generalized others in mind. The traits assessed by the X's, therefore, are regarded as referrable, hypothetically, to modes of regarding others. All else then follows. This is not to say that one learns nothing about the Y's in this way; but if one wants to learn about the emotions and temperamental qualities of the Y's there are appropriate ways to do this, such that the models used are at least to some degree isomorphic with material systems at issue, and this is not the case for Cattell's primaries, nor for Bank's factors, nor for Eysenck's hierarchical systems (Eysenck, 1953).

The demonstration I have provided should be regarded, perhaps, as a plausible account of the way meanings, in this case concerning personality, may develop and coadunate. Given the key modes A and B, pressing the two together reaches into AB—so the complex traits have meaning given to them, almost neurologically it would seem. Or, traits are so organized as to meaning. It only remains to indicate in what the deductive framework consists. The concern is entirely with *meanings*. In our factor scheme, I infer that the trait *serene* should be $(-A)(-B)(-C)$, that is, 'gracious' *plus* 'quiescent' *plus* 'independent'. *Cowardly* should be ACD, that is, 'naughty' *plus* 'immature' *plus* 'upsettable'. The opposite to *cowardly* is $(-A)(-C)(-D)$, that is, 'gracious' *plus* 'independent' *plus* 'tough', which could well be *brave* in the sense of 'carrying oneself defiantly under suspicion or blame'. It is not *brave* in the sense of *showy*, or a *desperado*, for which the factors should be BC, and $ABC(-D)$ respectively. All such inferences are carried out by myself, a sort of integrator; each inference, however, can be empirically tested, merely by including these various traits in future studies of the Cattell kind. The predictions can only be as good as the interpreter is sound as an integrator. The formal deductive framework provided by the factor model, even so, is a decided help in the right direction, without which no predictions would be possible.

X. CONCLUSION

The self-same data have been differently interpreted by Cattell, Burt, Banks and Eysenck. All place the factors in the Y's, the persons who are observed and rated by the X's. I place the factors, instead, in the X's. Those who look to factor analysis for scientific guidance may be pardoned, therefore, perhaps, for supposing that statistics can prove anything. However, the troubles are not statistical but logical and methodological. There has to be care for the material system at issue, for at least an approximation to isomorphism when this is covered by a model or metasystem, for what is operational and what not, for fruitful *ad hoc* explanations rather than merely explicit definitions, and for a genuine theoretical framework rather than for analysis into unrelated fragments of factors. In the above example the material system concerns modes of regard, generalized others, meanings, and the possibility of coadunation 'in the brain'. Factor analysis is a formal model. The theory, that simple modes of regard form first, and that from these complex traits are linked in a relay-key fashion, is highly tentative, fluid, an open invitation for further work, rather than a firm conclusion about anything. However, all one's interpretations can be empirically tested, which is a sound step indeed in any scientific work.

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SOME CHARACTERISTICS OF GOOD JUDGES OF OTHERS*

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A group of forty subjects, undergoing a 'living-in' assessment programme, were tested on their ability to make accurate analytic judgements about their peers. This ability was found to correlate positively with certain background variables such as urban origin, previous working experience, none or few siblings, being a student in the natural sciences (versus social science), and certain cognitive abilities. The good judges, as opposed to the poor ones, tended to be stable, serious and socially passive persons, task oriented and socially detached. No relationship was found between ability to judge others and artistic ability, ability to play roles, sociability, femininity or ability to judge self. The findings are explained in terms of a special ability, the availability of appropriate judgemental frames of reference, and the presence of motivation towards making objective social judgements. The good judges are typically extraceptive and the poor judges intraceptive.

I. INTRODUCTION

There is already an extensive literature pertaining to the ability to judge others, but much of it contains contradictory findings. This literature has been summarized by the writer (Taft, 1955) incorporating the findings presented below, and it will not be repeated in detail in the present report. The experiment described in this paper did not aim at testing hypotheses deduced from general psychological theory; its modest aim was to re-examine some of the long-cherished beliefs about the ability to judge others, to explore new areas of its correlates, and at the same time perhaps to reconcile some of the contradictions between previous studies. It is hoped, of course, that the empirical findings will indirectly advance the frontiers of knowledge about the operation of personality in general, and some suggestions along these lines are made in the discussion at the conclusion.

The experiment to be described resembles most nearly the study by Adams (1927), although techniques have been adopted from later experiments of a similar nature. The uniqueness of the present study lies in the fact that the judges were subject to an intensive personality investigation, and that, consequently, many more measures than usual were available for correlation with their ability to judge.

One limitation in the present study that should be borne in mind when evaluating the findings is the specialized type of judge used. It may well be that some of the characteristics found to discriminate between good and poor judges amongst advanced post-graduate students would not so discriminate amongst another sample of the population. It will be clear, however, that many of the findings of this study may be extended in their application far beyond the limited type of subject used here.

II. SUBJECTS AND ASSESSMENT CONDITIONS

There were forty subjects consisting of four equal groups. Each group of ten were the subjects of a large-scale personality assessment investigation, and for that purpose they

* The writer conducted this study while he was a research assistant at the Institute of Personality Assessment and Research, University of California, Berkeley. He extends thanks to his colleagues at the Institute, and especially to Dr Donald W. MacKinnon, for making data available and for their advice and criticism. The study on which this paper is based has been reported in more detail in Taft (1950).

were in residence together for a week-end. The subjects were all male graduate students either on the verge of completing the senior degree in their course or recent Ph.D. graduates. They included students in the physical and biological sciences, social sciences, law, business administration and music. The age range was 23–35, with a mean of 28 years.

Few of the subjects were acquainted with one another prior to the assessment programme—only 15% of all possible pairs had known each other before—but during the course of the week-end they were afforded considerable opportunity to become acquainted. The tests of ability to judge others were administered at the end of the assessment programme after the subjects had been living together for almost 2 days.

III. THE TESTS OF ABILITY TO JUDGE OTHERS

(i) *Estimation of M.M.P.I. responses*

The subjects were presented with thirty M.M.P.I. items and were asked tc predict 'how many of the ten assessees today, including yourself, answered each question the same as yourself'. All questions required the subject to answer 'true' or 'false'.

This technique has been adapted from that of Travers (1941), who did not use the method specifically as a test of ability to judge other people.

Scoring. A *discrepancy* score was computed by totalling the errors made in the estimates for each item without regard to sign; thus the lower the discrepancy, the greater the accuracy.

Reliability. It was not possible to apply the test-retest check on the present subjects, but this was done on thirty-three students of social psychology tested twice at an interval of five days. The test-retest correlation for the discrepancy scores of these subjects was 0·82.

(ii) *Rating test*

The most widely used single method of testing ability to judge others has been the rating method, since this method possesses the advantage of forcing the judge to express his judgements in terms that enable his accuracy to be computed quantitatively.

Instructions. The subjects were required to rate their nine-fellow-assessees and themselves on the six traits described below. A five-point scale was to be used according to defined standards; for example, a score of four means 'has above average amount (in the top 30% of male graduate students)'. The traits were defined as follows:

Persuasiveness: Ability to persuade others to agree with and to carry out his suggestions or points of view.

Social assertiveness: Takes initiative in making suggestions and giving orders; forceful and does not readily submit to another person (but need not necessarily be persuasive).

Sociability: Seek company and interacts readily.

Carefulness: Not impulsive; weighs the issues thoroughly before making decisions and taking actions and is slow to take chances.

Drive: Willing to undertake ambitious tasks and at the same time would show concentration and drive in executing them.

Conformity: Tries to conform closely to the conventions and expectations of his group with respect to appearance, social behaviour and attitudes.

Scoring. Accuracy was measured by the total discrepancy for all six traits between the judge's estimates and the subjects 'true' ratings. The problem, of course, is to establish the 'true' rating. For this purpose, discrepancy scores were computed using two separate criteria.

A. *Staff rating criteria.* After the conclusion of each assessment, the ten staff members who had observed the assessees rated them on the rating form. These ratings were then pooled and the means computed. This method of deriving pooled judgements has the disadvantage that it attenuates the range of the ratings, and extreme ratings are thus very rarely obtained. To compensate for this, the mean ratings for each trait were forcibly distributed for each group to correspond with the distribution of all of the individual staff ratings for the group on the particular trait concerned. These distributed ratings were then used as the 'staff rating criteria'.

B. *Peer rating criteria.* The *median* ratings on each assessee that were made by the other nine assessees were taken as the 'peer rating criteria'. The *mean* gives too much weight to extreme ratings possibly made by very poor judges, while the *mode* has the disadvantage that some of the distributions were bi-modal.

C. *Adjusted ratings—staff rating criteria.* The discrepancies on which this third index were based were computed after the judge's ratings on each trait had been adjusted to compensate for any systematic bias. This adjustment was made by adding to or subtracting the appropriate integer from all ratings made by a judge on a trait where his average under or over estimation per ratee was greater than 0·5. For example, if judge A's ratings of his nine subjects on Persuasiveness showed a total overestimation of 16, all of his ratings on this trait would be decreased by two and the discrepancies then re-computed. Wherever an adjustment was not possible owing to the ratings already being extreme (1 or 5), no adjustment was made.

It may be argued that since persons differ in the ease with which they may be judged, some of the judges had an easier task than others. For example, a person who was himself particularly difficult to judge would have an advantage over his peers in judging the others, since his peers' subjects include him. In order to compensate for this, the discrepancy scores obtained by each judge on each of the above three methods of scoring were adjusted for the 'judgability' of the subjects that he was judging. To make this adjustment, the discrepancy scores of each judge were divided by an index of the disagreement between the judges in judging each subject. Where a subject was difficult to judge the denominator in the formula for adjusted scores would be large, and thus the errors of a judge in rating him would not be weighted as heavily as his errors in rating a subject who was easy to judge.

The rating test provided three scores for each judge, each score representing his degree of accuracy in relation to the judgability of the subjects. These scores were: A, discrepancy —staff rating criteria; B, Discrepancy—peer rating criteria; C, Discrepancy—adjusted ratings.

Reliability. No retest was practicable within the assessment situation, and a split-half test would have rendered numbers that were too small. We were, however, able to rule out the possibility that the scores obtained could have occurred if the ratings were distributed over the five points of the scale according to a purely chance distribution. This possibility could fit our results less than once in a thousand cases.

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(iii) *Mosaic matching test*

During the course of the assessment week-end, the assessees were required to produce a mosaic design. These designs were put on display and the assessees were asked to match them with the 'artists'. They were allowed three matchings for each design and their score was the number of correct matchings.

(iv) *Overall index of ability to judge others (J.O.)*

The intercorrelations between the five measures described above are set out in Table 1. It should be noted, of course, that the correlations between the scores derived from the rating test are spuriously high since they are all derived from the same raw data.

Table 1. *Intercorrelations of measures of ability to judge others*

($N = 40$)

	1	2	3	4	5	Mean correlation
1. Estimation of M.M.P.I. responses	—	21	36	11	00	17
2. Rating test—Index A	21	—	72	81	07	45
3. Rating test—Index B	36	72	—	52	19	45
4. Rating test—Index C	11	81	52	—	11	39
5. Mosaic matching test	00	07	19	11	—	09

The mosaic matching test was omitted from the overall index, since the type of judgement required appeared to involve the use of non-analytic cues, whereas the other indices were more likely to depend on analytic cues. The estimation of M.M.P.I. responses test was included, since it appeared to test one facet of the ability to make analytic judgements about one's fellows, and it has a mean correlation with the other indices of 0·23 (omitting the matching test—P is 0·10).

The estimation of M.M.P.I. responses test was given a weighting of two, and the three rating test indices each a weighting of one. The total provided the J.O. measure (ability to judge others). Prior to totalling the above four scores, they were all converted to a normalized five-point scale so that their dispersions would be equivalent. This procedure may appear to coarsen the value of the J.O. index as a measure of ability to judge, but the four scores of which the index is composed are probably too unreliable to warrant a finer measure.

In the remainder of this report the term 'good judges of others' will refer to the thirteen subjects who obtained a score of 17 or more on J.O. (possible 25), and the term 'poor judges of others' will refer to the twelve subjects with a score of 13 or less. For convenience, the J.O. scores were converted into T scores with a mean of 50 and a S.D. of 10.

IV. RESULTS

(i) *Sociological and life history factors*

The small number of subjects makes it impossible to take into account interactions between the variables. These may be important, e.g. the subjects with a rural background tend to have more siblings than the urban subjects.

A. *Age.* The age range was only 23–35, and within these limits no differences in accuracy of judging were detectable for different ages.

B. *Socio-economic status and vocational experience.* There was no relation between ability to judge and present socio-economic status. Those who had been officers in the armed services tended to be superior judges to the others (not significant), as did those with full-time working experience (*P* 0·08). These tendencies may be related to social sophistication.

C. *Family background and minority status.* Those judges with an urban background were superior (*P* 0·03) to rural and small town, again possibly a function of social sophistication. Within the urban group the lower the status of father's occupation, the better the ability to judge (*P* 0·03). This can be explained in terms of the biased sample of judges being used: those from a poorer economic background who reach graduate school are a highly selected group; also they may have made a special effort to learn how to appraise others as a means of raising their own status.

The results for judges from minority groups throw light on the latter argument. Although the eight foreign students and Negroes were poorer judges than the others, the nine Jewish judges were significantly superior (*P* 0·02). Possibly the factors connected with ambition in the case of persons of lower status are operating here too. Jews have had a long history of cultural adaptation, and they may have developed modes of parent-child interaction that facilitate this process; they are also aided by their status both 'in' and 'outside' the prevailing culture. On the other hand, other minority group members without this tradition are under a handicap in judging their peers, most of whom are members of subcultural groups different from their own.

D. *Sibling status and parental press.* The good judges had an average of 1·4 siblings, while the poor judges had 2·8. The scores on the J.O. index are given for various groups in terms of number of siblings in Table 2. We see a definite tendency for ability to judge others to decrease when the number of siblings becomes more than three. Probably children brought up in large families associate mainly with their siblings and have less opportunity to learn adult modes of judging others. In this connexion we may note that Halmos (1952), in his study of solitude, found that adults who had had more than two siblings were more sociable than those with fewer siblings.

Table 2. *Relationship between ability to judge others (J.O. index) and number of siblings, sibling status and academic specialization*

	No. of siblings			Sibling status				Academic specialization	
J.O. index	0	1–3	4–6	Only	Oldest	Middle	Youngest	Physical and biological sciences	Social sciences
Mean *T* score	55·5	51·5	39·3	55·5	52·0	49·8	44·6	52·8 (56·2)*	47·0 (47·2)
No. above median	3†	16†	0	3†	9†	5	2	14 (14)	4‡ (4)
No. below median	0	12	7	0	5	7	7	5 (2)	13 (12)

* Figures in parentheses exclude foreign students.
† One case fell at median in each of these groups.
‡ Two cases fell at median.

This explanation is supported by the further findings in sibling status. There is a steady decline in ability to judge from only children to youngest (*P* 0·01). Apparently the greater degree of self-reliance, combined with the increased contact with parents enjoyed by only and oldest children, develops in them a capacity for appraising others such as is not developed by children with older siblings.

The findings with respect to family relationships, as far as they go, enable us to develop this point further. When asked to characterize the dominant attitude of their parents towards themselves, the good and poor judges differed in the responses which they gave about their mothers (but not their fathers). The good judges tended to characterize their mothers as over-solicitous, anxious, nagging and ambitious (for the subject) much more often than did the poor judges. There were twenty-five such entries for the thirteen good judges against only ten for the twelve poor judges. The fact that they cognized their mothers in this way is important to an understanding of the pressures upon them to learn to understand others in a realistic fashion. It is possible that the poor judges would have characterized their mothers in the same fashion as the good judges if they had learnt to appraise their parents objectively; but the fact that they may not have learned to do so could have resulted from a comparatively greater dependence upon their siblings than upon their parents. It might well be that the foundation stone upon which the ability to judge others accurately is built in the acquiring, at an early age, of the ability to appraise one's parents objectively.

(ii) *Academic specialization*

In view of the challenging findings (Luft, 1950) that physical science students tended to be better judges of others than psychologists, it is interesting to compare our physical science judges with the others. There were no psychology students amongst the assessees, but the physical (and biological) science students were significantly superior in judging to the social science students (P 0·01). In this regard it is interesting to note that Roe found that prominent physicists tended to be elder children (Roe, 1951, p. 130), and also that they tended to be socially isolated.

In fact only three of the twelve poor judges, were physical scientists, and of these, two were foreign students and the other was the only physical science subject who had not yet passed his Ph.D. preliminary examinations. On the other hand, there were only three social scientists amongst the thirteen good judges.

These findings support Luft's findings on the high ability of physical scientists to judge others. There apparently are some factors that determine the choice of one's occupation that are also related to the ability to judge others. Here we have our first suggestion of a characteristic attitude which will be noted in good judges of others in several further references. This attitude is one of dealing with people as objects of study in an objective fashion analogous to the study of inorganic or organic matter.

On the other hand, the poorer judges of others may have entered social sciences because they are interested in people *per se*. As we shall also have further occasion to mention, the poor judges are characterized by a dependence upon people for their emotional satisfactions and are thereby moved to judge them somewhat autistically.

(iii) *Cognitive abilities*

The correlations between ability to judge others and various measures of cognitive ability are set out in Table 3. Although none of them are significant at more than the 0·05 level, they all point to a low positive correlation. An index was computed to combine the P.M.A. and the cognitive tests in terms only of each subject's best performance—one good performance is indicative of 'g'. The scores on this index correlated +0·37 with the J.O. index (P 0·02).

(iv) *Academic ability*

Further support is given to the above picture when we consider ratings of the assessees' 'academic ability'. These were obtained by pooling ratings made of the assessees by their own departmental faculty members on two separate qualities: Potential Success ('likelihood that he will, in future years, contribute significantly to his field') and Originality ('his freshness of vision and creativity of thought'). The correlations of these ratings are 0·18 (0·30, omitting foreign students) and 0·12 respectively.

Table 3. *Product-moment correlations between ability to judge others and certain cognitive, artistic and dramatic abilities*

($N=40$, r of 0·31 significant at 5% level.)

Test	Correlation with J.O.
Certain cognitive abilities	
Primary mental abilities (word knowledge + reasoning + no. reasoning)	0·13
Miller Analogies Test	0·26
Gottschadlt Figures	0·29
Street Gestalt	0·11
Puzzles*	0·28
S's best performance	0·37
Artistic and dramatic abilities	
Barron–Welsh Figure Preference (3)	−0·19
Gerard Test of Good Taste (4)	0·31
Aesthetic quality of mosaic production†	0·21
Importance of aesthetic values†	−0·16
Understanding of role played in role-playing test†	−0·03
Empathy with role played†	−0·06

* These puzzles were a combination of verbal and practical problems requiring the use of 'insight' for their solution—designed by R. S. Crutchfield and P. F. Dempsey.

† Rating by assessment staff.

In summary then there is a consistent low, but not statistically significant, positive relationship between various cognitive abilities and the ability to judge others. At the same time it should be remembered that our group is particularly highly selected on intelligence.

(v) *Artistic and dramatic ability*

A number of artistic tests were given to the assessees, and the correlations are included in Table 3. The Gerard Test of Good Taste, the only test to show a significant correlation, requires the subject to choose between simple abstract figures (Gerard, 1946). The criterion of good taste used by the author of this test was the judgements of 300 'experts' (probably commercial artists, although this point is not clear) modified in the light of those responses which served to distinguish college graduates and students from persons of lesser education.

The results reported in Table 3 suggest that the ability to judge others correlates positively with ability to follow simple traditional artistic rules (Gerard Test and Quality of Mosaic Productions as assessed by lay judges), but not with a more sophisticated and labile artistic attitude. It might well be that professional artists would perform poorly on tests of ability to judge others.

Similarly, no correlation is found between ability to judge others and measures of the assessees' ability to understand and empathize with certain assigned roles in a role-playing test.

(vi) *Personality traits*

A. *Manifest personality.* The assessees rated themselves on a check list of 279 descriptive adjectives (prepared by H. G. Gough), and at the conclusion of each assessment period the staff also rated them on the same check list. Using these latter ratings as the criteria the following differences were found.

Those adjectives that characterized the good judges significantly more often than the poor judges: 0·01 level (0·001 level in italics):

alert, calm, capable, *cautious*, *clear-thinking*, efficient, honest, intelligent, *logical*, organized, persevering, planful, practical, quiet, *realistic*, reliable, *reserved*, *serious*, *sincere*, *thorough*.

Supplementary list significant at 0·10 level:

aloof, awkward, conscientious, conservative, cool, dependable, determined, fair-minded, gentle, humourless, independent, industrious, moderate, painstaking, precise, rational, reasonably, unaffected.

Those adjectives that characterized the poor judges more often; 0·01 level:

affected, *argumentative*, changeable, demanding, excitable, hasty, headstrong, imaginative, irresponsible, obnoxious, opinionated, outgoing, pleasure-seeking, self-seeking, show-off, suggestible, *talkative*, *unstable*, undependable.

Supplementary list significant at the 0·10 level:

careless, civilized, dependent, dominant, egotistical, emotional, high-strung, humorous, immature, individualistic, simple, sophisticated, undependable, wary, witty.

Thus we get a picture of the good judge of others as a serious, organized and reasonable person who apparently relies upon the use of his intelligence and conservatism in meeting successfully the hazards of life. On the other hand, the poor judge is socially oriented, but not socially adjusted.

Support for this picture is offered by the subjects' own checks on the adjective list. The good judges prefer to use terms that indicate achievement orientation at the expense of social relations, e.g. industrious, logical, painstaking, reserved and retiring. The poor judges preferred adjectives almost all of which reveal social orientation, e.g. noisy, show-off, egotistical, emotional, affectionate, clever—but also careless.

B. *Masculinity-femininity.* Four measures of MF were obtained (M.M.P.I., M.N.E., (Gough, 1952), Franck Figure Preference Test (Franck, 1946) and Franck-Rosen Figure Completion Test (Franck & Rosen, 1949)), and none of the correlations with J.O. was significantly different from zero (range 0·00 to −0·12).

C. *Emotional stability.* We have already noted that in the Adjective Check List, the good judges were more often characterized as organized and stable. This is supported by overall ratings on Personal Soundness ('general effectiveness as a person, with emphasis on maturity and balance'). Ratings on this trait by the assessment staff correlated 0·29 with J.O. and 0·37 excluding foreign students (*P* 0·02). Ratings by the assessees' departmental faculty members correlated 0·29 and 0·43 respectively (*P* 0·01).

A test of ability to find the vertical in a dark room* correlated 0·32 (*P* 0·05) with J.O.

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* Based on the Asch–Witkin technique (Witkin & Asch, 1948) modified by R. S. Crutchfield.

If the suggestions made by Witkin (1949) are to be accepted, this would indicate that the good judges are the more self-sufficient and stable.

D. *Self-insight.* If we define self-insight as the ability to judge oneself accurately it is possible to compute a J.S. index corresponding to the J.O. index. This index included measures of the subject's ability to rate his own traits accurately, to predict his standing on the M.M.P.I. items in relation to the group's responses, and to predict his sociometric peer status. A correlation of only 0·11 was found between these two indices, suggesting that there is no relationship between the ability to judge others and self-insight. Actually the relationship is a complicated one and is non-linear; the poor judges of others tend to be either good or poor judges of themselves, while the good judges of others tend to neither extreme. Six of the twelve poor judges of others show what might be termed an inadequate degree of self-insight in comparison with only one of the thirteen good judges.

(vii) *Social relations*

A. *Attitude towards others.* We have already observed in connexion with the Adjective Check List that the poor judges of others appear to be more concerned with qualities relating to social relations, while the good judges are more achievement oriented. It has been suggested that these and other differences are indicative of a socially dependent attitude on the part of poor judges and a socially detached one on the part of good judges. Some further support is provided by the negative correlations between ability to judge and ratings by the assessment staff on exhibitionism (−0·26, *P* 0·10) and sociability (−0·22, *P* 0·20).

Much is revealed about social attitudes if we consider that the poor judges were much more likely to make errors in the direction of over-generosity than were the good judges. To measure this tendency, an index of generosity was computed according to the degree to which errors made in the rating test were errors of over-rating or of under-rating. (It was assumed that a high rating on all of the six traits—persuasiveness, social assertiveness, sociability, carefulness, drive, conformity—was a favourable one, although this is open to doubt in the case of conformity.) The generosity index correlated −0·48 (*P* 0·01) with J.O. This suggests that there might be unwillingness on the part of the poor judges to risk removing their social supports by perceiving other people as they really are.

It is interesting to note that the difference in attitude towards their own social relations that we have detected in this and the previous section does not appear to carry over into a like or dislike of the company of others. The good judges tend to belong to as many social organizations as do the poor judges. Also there is no difference in the number of friends claimed by the two groups. Apparently the good judges of others enjoy contacts with other people and are by no means lacking in experience of such contacts. We are reminded of the distinction made by Asch between 'social interest' and 'social feeling' (1952, p. 335).

B. *Social skill.* Since the poor judges are more motivated to manipulate others to achieve their own purposes, particularly to receive recognition, it is of interest to note with what success they attain this. Judging by the Adjective Check List the poor judges do not seem to be any more successful than the good ones in attaining harmonious social relations. The good judges at least manage to avoid being characterized as 'obnoxious'! Ratings by the staff on social ascendancy, social relations and persuasiveness are not correlated significantly with J.O. (−0·08, +0·13 and −0·11 respectively). Sociometric choices by their fellow-assessees on likeability as a friend correlates 0·03 with J.O. The only instance in which any significant correlation was found between J.O. and Social Skill was the rating on 'ingenuity' in the role-playing test where the correlation was −0·33. This correlation was probably a product of the poor judges' exhibitionism which led to their structuring their assigned roles in an original manner. In general, we can conclude that there is little relation between ability to judge others and social skill; certainly there is no evidence that high ability to judge other people is an aid in handling them.

V. Conclusions and discussion

The correlations reported with ability to judge others may be explained in terms of one or more of three variables: cognitive ability, appropriate frame of reference and social attitudes. Cognitive ability barely enters into the picture in the highly selected group used here, but it appears to have some relevance; and this is only to be expected since an intellectual judgement is required. Frame of reference is suggested in the finding that rural and minority groups, and those without working experience, are poorer judges—they might easily have more 'ethical inhibitions' about judging their fellows as well as fewer opportunities to acquire the appropriate frames of reference. The same can be said of those subjects who were younger children. Also the poor judges tend to be less stable and organized, and they probably have less stable frames of reference.

Social attitudes, however, appear to be the most important variable; poor and good judges have been shown to vary in the degree to which they are socially dependent or detached, and this difference can explain the differences found in sibling status, academic specialization, and tendency to be a generous rater.

Apart from sheer experience and ability, the ability to judge others can be thought of in functional terms—how it fits in with the judges' motivations and attitudes towards judging his peers. The typology which we have deduced for the good and poor judges of personality can be expressed in the terms used by Murray (1938, pp. 222–3). Our good judges are his endo-cathective + extraceptive type, not intraceptive as he seems to suggest; both intraceptive types appear to correspond with our poor judges.

An interesting comparison with our typology of good judges is provided by a group of subjects described by Pemberton (1952) as a 'high flexibility of closure' group. These subjects were typically physical science students; they scored well on the Gottschaldt Figures, were inclined to be socially detached and denied that they were 'intuitive'. Flexibility of closure seems to describe one of the characteristics required in the act of making a considered analytic judgement about an object or person, and the similarity between Pemberton's results and our own are striking with one exception—her subjects are described as not 'systematic'.

In conclusion, I should like to suggest a possible explanation for the particular pattern of characteristics found in good judges of others. These characteristics are all ones which befit a person to adjust to our present technological-scientific ethos—physical scientists, intelligent, organized, stable, hard-headed, administrative, socially detached and not intuitive types. The explanation is that judging another person in analytic terms, treating him as an object, is just another manifestation of this ethos. But whether this explanation is sound or not is a question which will probably be illuminated more by anthropological evidence than by psychological.

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TOWARDS AN INFORMATION-FLOW MODEL OF HUMAN BEHAVIOUR*

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This paper is concerned with the behaviour possible in an information-flow system intended explicitly as a hypothetical model for comparison with the human information-handling system. A statistically self-organizing system is described in which not only normal homeostatic behaviour but also such activities as the invention of fruitful hypotheses, the imagination of fictitious situations, and the like would find a natural place. Discussion is confined mainly to the manner of concept-formation and concept-handling in such a system. It has been suggested that the correlate of perception (as distinct from reception) is activity which organizes an outwardly directed internal matching response to signals from receptors. This organizing activity amounts logically to an internal representation of the feature in the incoming signals to which it is adaptive, i.e. the feature which is thus 'perceived'.

A hierarchic structure is postulated wherein much of the organizing activity is concerned with modifying the probabilities of other activity. Abstract concepts and hypotheses are represented by 'sub-routines' of such organizing activity. These can in principle be evolved as a result of experience in a manner analogous to—or at least fruitfully comparable with—the process of learning and discovery; and it is not necessary for the designer to predetermine, nor possible for him to foresee, all the conceptual categories in terms of which the information received by the system may be structured. Particular attention is directed to the conditions under which such a process of concept-formation could take place with sufficient rapidity.

Some of the symptoms of psychopathology find apparent correlates in possible modes of malfunction of such a system. But in this, as in other respects, it is not intended to press the resemblances; the intention is rather to stimulate critical comparisons, in order that the differences between the information-flow model and the real thing may continually lead to progressive refinement and enlarged understanding.

I. Introductory

(i) *The wrong approach.* I suppose that the most superficial and unconvincing excuse for agreeing to discuss the behaviour of any artificial mechanism before a gathering of psychologists might run somewhat as follows: Experimental psychology has lately come to concern itself more and more with the human being as a 'black box', having certain characteristic ways of receiving and reacting to information (Hick, 1954). Perhaps not altogether accidentally, the merchant of automata has discovered that in his stock list there are several artificially contrived black boxes which in principle can be made to receive and react to information in the same characteristic ways. So, he might argue, his artificial black boxes are surely quite as proper subjects of the psychologist's attention as the human ones.

This argument seems to me, putting it mildly, to be mistaken. If the psychologist had *defined* his field as including 'all black boxes with a certain behaviour pattern', the excuse might hold. But if, as I believe, he defines it to be human (or animal) behaviour, then the merchant of automata has no automatic claim on his attention or even his interest. An automaton giving the most perfect imitation of observable human behaviour might prove to hold as much interest for the psychologist as a gramophone capable of reproducing human singing does for the laryngologist, and no more.

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* Based on a paper read at the 14th International Congress of Psychology in Montreal, 11 June 1954.

(ii) *The conceptual role of the theory of automata.* So I must offer, tentatively, a rather different excuse. I believe that the theory of automata is potentially of use to the psychologist, not just because some automata could imitate the behaviour of his subjects, but because some aspects of the theory could, if properly disciplined, provide him with a useful tool of research.

What I think it offers is a kind of logical scaffolding, which may play a useful part in the processes of erecting, testing and (alas) demolishing psychological theories. We search nowadays for clues to the organization of human personality in two widely different fields, the provinces broadly speaking of psychology and physiology. These clues come expressed in characteristically different languages. Particularly in descriptions of pathological symptoms, it is often hard to imagine with any confidence the proper correlate in one language of data expressed in the other. We are convinced that the data *should* help us, but we have little idea what to make of them.

Now the language of information and control, in which the theory of automata is framed, is conceptually intermediate between those of psychology and physiology. It belongs in a sense to both fields, inasmuch as questions of information transfer arise in both.

It seems possible therefore that the theory of an automaton, specifically designed to be a research model of the human organism considered as an information flow system, might provide a common meeting-ground and a common language in which hypotheses might be framed and tested and progressively refined with the help of clues from both sides.

(iii) *The information-flow map.* For this purpose the physical engineering of the automaton is of little interest. It is sufficient if it can develop and grow on paper as a hypothetical information-flow map of the human organism. Just as the lines on a trade-route map of the world are drawn in the same way, whether goods are carried by train or lorry or camel caravan, so the pathways of our information-flow map may be postulated and tested and confirmed irrespective of the chemical, electrical or other details of the processes mediating the traffic in information.

On the other hand, the features of the information-flow map will normally suggest experimental questions in both the psychological and physiological fields, and should in turn find themselves tested and refined by the results.

This is what I meant by calling the theory of automata a logical scaffolding, potentially useful in building operations on psychological theories. Merely to ask what functions can be imitated, and how, would be pointless. The answer is that any pattern of behaviour that can be specified exactly or statistically can in principle be shown by an automaton, designed in one of any number of alternative ways (MacKay, 1951).

The real question is how far we can go towards designing a self-organizing system with the same internal information-flow map, as well as the same behaviour, as a human being.

We shall not find the answer in this paper. But I hope to show you that the answer is reasonable to seek, and not impossible to imagine, in dim outline, even now.

II. GOAL-GUIDED ACTIVITY

(i) *Minimal requirements.* Perhaps the most characteristic feature of intelligent behaviour is what we call goal-guided (goal-directed, adaptive, conative, purposive) activity. Let us begin by summarizing the essential features of any information-flow system that is to show such activity. These features are by now familiar to most of us, and I have discussed them in detail elsewhere (MacKay, 1951, 1952). The minimal requirements are shown in outline in Fig. 1, where the activity of the system is represented simply by the movement of a point Y along a line F.

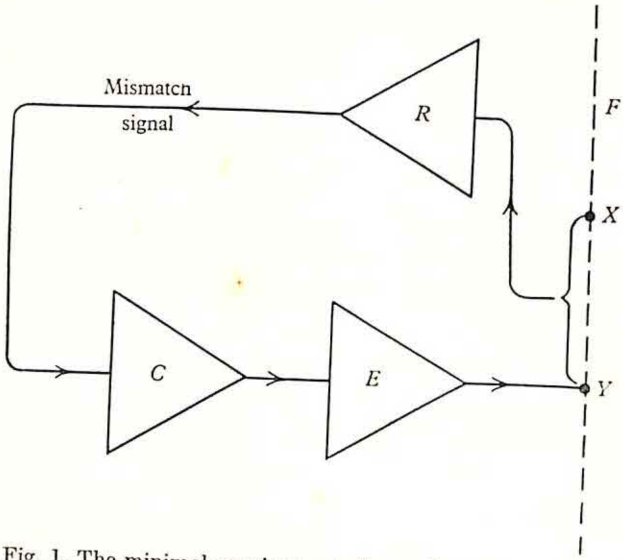

Fig. 1. The minimal requirements for goal-guided activity.

The active agent is represented by the 'effector system' E, which is governed by the control system C. We assume that in a typical situation E is capable of a certain range or variety of modes of activity (including inactivity), so that the function of C is to *select* from moment to moment what E shall do next, out of the range of possibilities open to it.

Suppose now that on the line F there is a point X, fixed or moving, towards which we want the activity of E to be 'goal-guided'. What do we mean by this? We mean (*a*) that the overall pattern of E's activity should be calculated to reduce the interval between X and Y, or some time average of it, to a minimum. We mean (*b*) that if the interval were increased in any way (e.g. by movement of X away from Y, or by the action of some external agency forcing Y away from X) the effector E should actively resist the increase; but that it should actively assist any agency tending to reduce the interval.

The first of these requirements could in principle be met by a 'blind' automaton with a pre-computed control sequence, if the motions of the goal were known in advance. But in order to cope with the unpredicted influence of external agencies, something more is needed. The controlling system C must *receive information from the field* about the interval XY, and use it to control (wholly or partly) the selection of what E shall do next. This information is represented in Fig. 1 as carried to C by a 'mismatch signal' from a receptor system R coupled with the field through any number of modalities.

(ii) *Improvement in performance.* On the very simplest system, only the existence of the interval need be signalled to C. C must then be designed to keep E in randomly or systematically varied activity until the interval is reduced below some acceptable level (the 'Homeostat' of Ashby (1952) is a typical example).

We could generally achieve greater speed and efficiency, however (at the cost of greater complexity in the mismatch-signalling system), if the direction—and still better, the magnitude—of the interval XY, and not merely its existence, were signalled back to C. In the extreme case we could provide C with a complete representation of the current state of the field of activity, from which to calculate automatically the best course of action for E.

If information can also be *stored* in C, its calculations can be extended to cover rates of movement and to make predictions on a statistical basis, so increasing still further the effectiveness of E's activity. For this purpose we do not need to picture a discrete store of precisely symbolized information as in a digital computor. 'Storage' here means any modification of state due to information received and capable of influencing later activity, for however short a time.

(iii) *The distinction between goal-oriented and goal-guided activity.* In passing we may note (see MacKay, 1952) that there are some physical systems in which the requirements for goal-directed activity (in one sense) seem superficially to be met without any of the features of Fig. 1. A steel ball, for example, will 'pursue' a moving magnet and resist attempts to deflect it.

But in all systems of this sort, the essential difference is that *no other* activity is physically possible to the active agent in the given circumstances. The question of 'guidance' or 'direction', in the sense of a choice among physically open alternatives, does not arise. The required activity is in a sense uniquely implicit in the physical description of the situation requiring it. This is even more readily seen if we consider a physically equivalent model in which the ball is attached to the magnet by an elastic string. Its actions are merely 'newtonian' reactions to the situation in which it is placed.

It would, in fact, be rather tendentious to describe the magnet as the 'goal' of the ball. But if we must use the term, it might be better to avoid confusion by describing such activity as 'goal-oriented' rather than 'goal-directed'.

(iv) *Examples of goal-guided activity.* The simple flow map of Fig. 1 may be taken as typical of all situations in which goal-guided activity is required. In the simple thermostat, for example, the effector E is a heating unit. The field of activity F is the scale of temperature, on which the present state of the system is represented by some point Y, and the goal state by some pre-assigned point X. Information as to the existence and the direction (and sometimes the magnitude) of the interval XY is provided by some kind of thermometer (combining the functions of R and C) which serves to *compare* the present temperature with the goal temperature, and mechanically adjusts the activity of the heating unit in a direction calculated to reduce the discrepancy. In more complex cases the effector E may have many degrees of freedom (for example, it may have the equivalent of a large number of independent muscles), and its field of activity F will then be represented not by a one-dimensional line but by an abstract multi-dimensional space with a corresponding number of dimensions. In this space, however, the present state and the goal state will still be represented by points, X and Y, whose co-ordinates will be the values of the corresponding parameters. The magnitude and direction of the interval XY will have the same significance as in our one-dimensional illustration, and the system will work on exactly the same principles.

It will be clear from this example that in general the points X and Y in Fig. 1 do not represent physical *objects* in the environment of A. The point Y is an abstract way of representing the current *state* of the total situation, system plus environment. The point X similarly represents the state of the total situation towards which as goal the activity is guided.

By defining X and Y in this way as representing states and not physical objects, we can make the same formal description apply equally well to a self-guided missile chasing an aircraft, and a man chasing the solution to a crossword puzzle.

(v) *Towards inventive behaviour.* On the basic principle of Fig. 1, there is no difficulty in envisaging an automaton capable of interacting actively and apparently purposefully with its environment and in particular with human beings, in any field of activity for which it has appropriate effectors and receptors, and an adequate symbolic calculus embodied in its controlling element.

But there, of course, is the rub. It is true that human beings are educated in childhood to use a standard terminology, embodying standard categories of perception and description. To this extent we might be entitled to preform the categories in terms of which our model automaton structures its information. But human beings do more than carry out logically deductive operations in a fixed calculus. They invent new categories and novel hypotheses, to meet an unforeseen flux of events. They imagine unrealized situations....

How much farther can we go with our automaton? What kind of automaton, if any, could evolve for itself new descriptive categories appropriate to situations that its designer had not envisaged—and do it in a way analogous to the way in which a human being does? What place can we find in the theory of automata for such human activities as the invention of relevant and fruitful hypotheses, or the contemplation of imaginary situations?

In what follows I want to suggest, very tentatively, some principles on which a self-organizing system could be devised to parallel many of these inventive features of human activity—principles which at least bear realistic comparison with those so far recognized by psychologists in the development and functioning of the human information-flow system.

III. ORGANIZING THE TRIAL PROCESS

(i) *Simple groping.* We have already seen that the system of Fig. 1 could show goal-guided activity even with the minimum of information about the interval XY. In the limit, it could simply run through its repertoire of activity until the mismatch signal disappeared. But if its repertoire is at all large, this process takes time. If the effector system E has l independent degrees of freedom, each associated with m alternatives, the number of combinations which might be tried, and hence the average time of hunting, rises as m^l. Thus this groping automaton cannot hope to cope with a complex situation in which the goal-state changes rapidly and unpredictably. Under such conditions it would do little worse if the mismatch signal were dispensed with altogether,* since even if by chance the current goal state were reached and XY reduced momentarily to zero, it would probably not remain so for more than a small fraction of the time spent in hunting.

(ii) *Directional guidance.* Things would be much better if the mismatch signal could indicate the *direction* of the interval XY. Even if only the *sign* of each component of the vector interval were signalled, the improvement could be enormous. Each trial (for an l-dimensional interval) would then provide approximately l independent yes-no decisions or (in the jargon of information theory) 'bits of information'. This could ideally reduce the minimum number of trials to only $\log_2 m$, i.e. $(1/l)\,(\log_2 m^l)$.

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* The Irishman's clock, we remember (which had stopped), succeeded in telling the time correctly twice a day.

But with a complex effector system having many degrees of freedom, even this information capacity may be too much to ask of the mismatch-signalling system. This is not the place for a discussion of the informational requirements in detail; a start has been made in another paper (MacKay, 1955). But we must evidently ask at this stage whether there is no way of improving the performance of C without demanding from the mismatch signal the full selective-information content of $l \log_2 m$ bits which is ideally required in order to select the correct adaptive response by E to each fresh movement of X.

(iii) *Making use of 'redundancy'*. Information theory assures us that in principle there should be such a way if the movements of X show any regularity in their statistical structure, or 'redundancy', as it is called. In that case, as in an ordinary communication channel, the average selective-information content of the correct response may be much

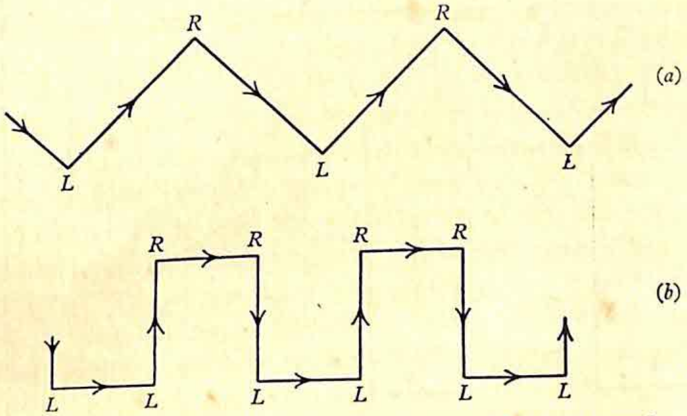

Fig. 2. Two typically 'redundant' sequences of bends in a road.

less than the $l \log_2 m$ bits which we first estimated. In practical terms, the trial process can afford to concentrate more on trying some combinations of activity rather than others; so on the average it can *save time*, or can reach the goal *in the same time with less information* from the mismatch-signalling system. Our problem is then to devise a way of profiting from the redundancy of the information sequence represented by the movements of the point X, or equivalently by the corresponding successful configurations of the activity of E.

(iv) *A simple example.* To take a simple example which I have used elsewhere (MacKay, 1955), imagine an automaton designed to run along a road consisting simply of a succession of straight runs and right-angle bends. The automaton has only two control buttons, L and R, which when pressed cause it to turn left or right respectively. When it reaches a bend in the road, a simple mismatch signal automatically induces random trials of L or R until the correct button is pressed and it moves on.

Now suppose that over a long stretch of road, L and R bends form a regular sequence, such as $LRLR$ (Fig. 2*a*), which we may call S_1, or $LLRRLLRR$ (Fig. 2*b*) (S_2). We could obviously save hunting time if we were now to install in the controlling system a simple button-pressing mechanism or 'organizer' which imitated the appropriate rhythmic sequence of bends in the road. For example, in the first case, this organizer would always respond to the mismatch signal by trying R if L had last been used, and vice versa. Thus every mismatch signal would at once induce the correct adaptive response without any hunting. The high degree of redundancy in the series of bends permits of a correspondingly low information content per mismatch signal.

(v) *Second-order control.* Consider now a more complex road comprising a mixture of sequences of the forms S_1 and S_2. Each time the sequence changes the internal imitative organizer must now make a corresponding change in the rhythm of control signals. Either its connexions must be altered, or its place must be taken by another mechanism suitably connected to generate the next rhythm.

We need, in fact, a second control mechanism, C_2 (Fig. 3), to organize trial routines from the repertoire of C_1 in the same way as C_1 organizes trial routines from the repertoire of E.

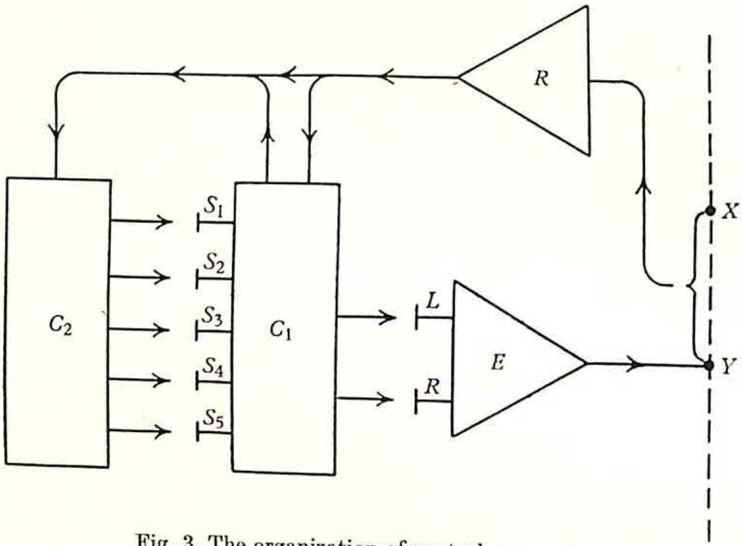

Fig. 3. The organization of control sequences.

If, for example, the road sequence changed from S_1 to S_2 and back again to S_1 in an alternating series, the organizer C_2 could be arranged to hunt through the repertoire of C_1 until it found the correct successor, each time that a succession of mismatch signals indicated that the road sequence had changed. But here again the efficiency could be increased, if C_2 incorporated a second-order button-pressing mechanism designed to try S_2 after S_1 and S_1 after S_2.

Obviously we can envisage an indefinite hierarchy of such imitative organizers. Each will represent by its activity an *abstraction* descriptive of some aspect of the series of bends in the road.

IV. A STATISTICAL SELF-ORGANIZING SYSTEM

(i) *The automatic development of organization.* Our last example was so far all very well; but it presupposed that we, the designers, foresaw in advance the range of possible shapes of the road. Our next step—the fundamental one for our present purpose—must be to devise some principle on which the system could discover for itself the statistically stationary features of its environment, and develop its own characteristic organizing sequences as a result of its experience, starting if necessary from scratch.

(ii) *Statistically adjustable links.* The most promising way of solving this problem—the problem of growing an organizing system from scratch—would seem to be to design the control mechanism out of statistically adjustable elements. By this I mean that instead of making its interconnexions hard and fast by means of switches that are either open or shut, we use 'statistical' interconnexions or links which have an adjustable *probability* of transferring information. This gives us all the flexibility we need. Since the 'thresholds' governing these probabilities may be continuously variable, the statistical structure of the activity pattern is capable of assuming an indefinite number of finely graded configurations, far larger than the number of combinations possible with simple on-off switches in each link. The information content represented by the current state of a given number of elements is correspondingly greater than in the simple case, so that the design can in principle be more economical of elements for a given required variability of activity pattern.

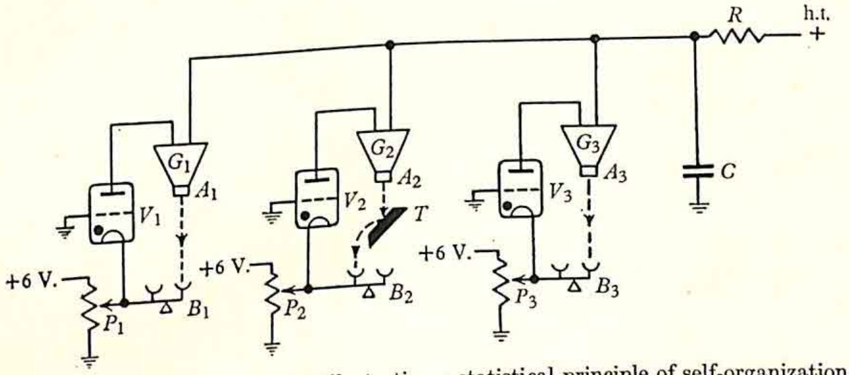

Fig. 4. A simple electronic device illustrating a statistical principle of self-organization.

(iii) *Control of statistical bias.* We can now envisage a self-organizing system in which the mismatch signal automatically adjusts the configuration of thresholds just mentioned, governing the statistical pattern of activity according to the success or failure achieved, so that successful sub-routines of activity become more likely to be tried again in similar circumstances, while unsuccessful ones become less likely to be tried.

Provided that the environmental world of activity shows consistent features, these could then in time come to be reflected in the relative probabilities of different organizing sub-routines.

(iv) *A simple example of a statistically self-organizing system.* An excessively simple illustration of this kind of process is provided by the automaton of Fig. 4. This device was built a few years ago to illustrate a lecture (MacKay, 1949). It has of course no pretensions to animal status, and justifies its existence solely as an explanatory device. It is made up of three identical units (the number is unrestricted in principle), each comprising a thyratron valve V which can actuate an electromagnetic gate G so as to drop a ball-bearing from an aperture A over a two-pan balance B. B is mechanically linked to a potentiometer P, which governs the bias on thyratron V. The anodes of all thyratrons are supplied from a common capacitor C charged through a resistor R.

If now C is allowed to charge slowly, the thyratron with the lowest effective bias, say V_1, will tend to fire first, discharging C and releasing a ball from A_1. Normally this will fall into the right-hand pan of B, causing P_1 to rotate and increase the bias on V_1. Thus the normal effect of a thyratron's firing is to reduce the probability of its firing next time.

If, however, a target T is placed under A_1 so as to deflect any balls from A_1 into the left-hand pan of B_1, the opposite effect will occur. The success of the automaton in striking the target when V_1 fires will increase the probability that it will use V_1 next time.

Evidently no matter how the biases on different V's are set initially, the automaton will tend to an equilibrium state in which only the thyratron V_1 fires and T is always hit. If, moreover, we move T to a new position, say no. 2, the automaton will gradually 'unlearn' its original 'set' and eventually will be found firing only V_2.

In this example randomness is introduced only by the fluctuations of the thresholds of the various thyratrons, and the mismatch signals are quantized and very simple. Even so it illustrates the basic principle, whereby an automaton can selectively 'grow' modes of activity according to its experience of their relative success, and diminish by a kind of natural selection the probabilities of all except those which have been successful often enough.

(v) *Higher-order statistical control.* When we come to apply this principle of statistical control to the system of Fig. 3, the situation is rather more complex. The actions whose probabilities are adjustable are not merely external effector actions, but actions of one element on another; in particular, they include actions which alter 'conditional probabilities' of action by other elements. For example, in order to set up the sub-routine $LRLR$ it is necessary that the action L shall modify the conditional probability, that R shall follow if and when the mismatch-signal next calls for action. There will thus be some situations of the general type of Fig. 1, wholly *within* the mechanism itself.

But the principle on which this system should be made self-organizing is the same. As before, we introduce a second organizer, C_2, working now on the same statistical principles as C_1 which we have just discussed. Metaphorically speaking, C_2 can be on the watch for any regularities in the series of different routines found successful by C_1. As these regularities develop, C_2 will spontaneously begin to grow patterns of activity to imitate them and pre-organize the probabilities of their occurrence, so reducing the average time spent in hunting through C_1's repertoire, and at the same time increasing the statistical stability of any closed cycles that C_1 finds to be adaptive. If the organizing mechanism begins with most or all thresholds fairly low, so that all kinds of sequence are possible, then after a long series of $LRLR$ bends in the road the probabilities of actions not contributing successfully to the trial process can be steadily reduced and the others reinforced until a firm pattern of organization emerges.

In fact we can envisage in this case, just as in paragraph III(v), a whole hierarchy of such organizers, whose activity has here developed from scratch a statistical pattern to match the stable structure discovered in the environmental sequence of events. Although the term 'hierarchy' suggests an ordered system of separate levels, the most flexible system would allow of activity from different levels to be mixed in each. We might even envisage an abstractive 'level' which took as its data the activity of the whole system of levels; and it may be profitable to investigate the results of allowing the activity of the 'top level' of the hierarchy to become the data for the 'bottom level', the mismatch signal being distributed initially more or less evenly throughout all levels. But the notion of a simple hierarchy is sufficient for our present purposes.

(vi) *The stabilizing principle.* Thus far we have spoken as if thresholds (and their corresponding 'transition probabilities') could be altered only by the action of the mismatch signal; as if 'adaptive success' were the only condition on which the probability of a given sequence should be increased. But there are several ways in which the adaptive efficiency of the system (in a normal human environment) could be augmented, thereby incidentally increasing its resemblances to the human organism.

Let us introduce a second principle of organization, according to which the probability of a given transition is augmented automatically (though not indefinitely) according to the frequency of its occurrence, in addition to any modification resulting from its success or failure. What effect will this have on the growth of organization? First, and obviously, it will mean that every time a routine of activity comes 'back to where it started', it will receive some reinforcement in addition to whatever evaluation, positive or negative, is deduced from the mismatch signal. Closed cycles of activity will tend to be statistically favoured. As time goes on the adaptive organizing responses most likely to be tried will be those closed cycles which appear to be statistically nearest to what is required, as deduced from the mismatch signal. The system, in other words, will have a tendency to construct its imitative organizing sequences out of the nearest approximate combination of closed cycles of activity; the system will tend to perceive its environment as a compresence of associated 'wholes'.

The second effect will be to increase the speed with which the organization will grow in a relatively stable environment where the same sub-routine may be repeatedly called for. Closed sub-routines once found successful are less liable to disruption in subsequent trial processes, and more likely to be retained, excited or suppressed as wholes, ready more rapidly for use when the corresponding stable feature recurs in the flux of environmental events.

A third result of applying this principle is that the shorter closed cycles of organizing activity will tend to be even more favoured than they would be in any case. The effect is in fact analogous to 'negative resistance' or 'positive feedback', in the jargon of control theory; but it is feedback exerted on the probability pattern rather than on the activity itself. It tends to favour the formation of metastable 'eigenstates' of organizing activity and readiness for activity. The bias of our system will be towards conceptual simplicity. The mark of Occam's razor will appear naturally on the pattern of its activity.

(vii) *The 'anti-monopoly' principle.* The third principle of organization of the threshold control mechanism that should be mentioned here might seem superficially antagonistic to our earlier requirements. It is a provision that the intensity of a given pattern of activity will tend to die away with time. This we might describe as an 'anti-monopoly' proviso. Its function is to preserve the exploratory character of adaptive activity by ensuring that the system does not settle down for too long in any one adaptive mode. It thus increases the number of features of the environment likely within a given time to find themselves represented in the activity pattern.

This feature, analogous in some ways to the familiar 'adaptation' of sensory organs, is actually in no way antagonistic to our second requirement that the *transition* probabilities for closed cycles should automatically rise with time. To take a simple example, both requirements would be met in a system based on the principle of Fig. 4 if the high-tension supply battery were subject to polarization, so that its e.m.f. dropped steadily as long as current was drawn from it, and recovered only after a period of rest. In Fig. 4, the *relative* probability of the successful thyratron's firing increases with each successful action. It is only the probability of its *action in a given time interval* that decreases with each action.

In general, then, we must distinguish clearly between the control of relative probabilities of alternative courses of action, and the control of the probability of a given action in given circumstances within a given time interval. The first determines the probability that, if an action occurs, it shall be of a given form. The second determines the probability that an action of this form shall occur (in a given time interval). Since the thresholds governing these probabilities are continually changing the probabilities cannot of course be interpreted as frequencies in an observable time series of actions, but (as in analogous situations in physics) only as the relative proportions, or frequencies, in an imaginary 'ensemble' or population of similar situations.

V. THE CONTROL OF DIMENSIONALITY

(i) *The speed of development.* By intelligent use of the foregoing principles, I think that a self-organizing system can be envisaged which could develop, *ab initio*, adaptive powers comparable with those of a human being, if subjected to a similar course of typical experience. But we have skirted one important practical question. Even with all our aids to efficiency, is there any hope of this development taking place in a *time* comparable with that required by an average human infant? Will not the number of possibilities to be eliminated, and the number of wasted trials, be astronomically large if the field of activity is to be as complex as the human one?

(ii) *Statistical control in the effector system.* I have only a tentative answer to this question, but it is one that seems promising. We have argued hitherto as if the degrees of freedom of the effector system E were large in number and fixed, so that each step in the process of development amounted to the elimination of all but one of a vast number of alternatives.

Let us picture now, however, an effector system constructed on much the same statistical lines as the control system we have discussed; and suppose that initially the elements are so tightly coupled together (by statistical links with low thresholds) that the effective number of independent degrees of freedom is very small. The responses possible will be relatively few and crude, but the initial trial and error processes of goal pursuit can be correspondingly rapid. Few effector actions will be highly successful, but the best in given circumstances will be fairly quickly discovered. The broad outlines of the statistical structure of the environmental events will begin fairly quickly to be reflected in the pattern of internal organizing activity.

Suppose now that as this happens, the degrees of freedom of the effector system are gradually and correspondingly increased in number, by a reduction in the statistical coupling between elements under the guidance of the mismatch signal. To take a simple example, the system might start with the controls of all of five 'fingers' interconnected so as to function as a single unit. As the organizing process went on, it could be arranged that the general level of the thresholds in the interconnecting links should rise, so that the 'fingers' would begin to gain independence; but any links found in practice to promote successful responses could be selectively favoured, and could retain a greater-than-average probability of functioning.

(iii) *Comparison with the human system.* In a suitable environment offering a sufficiently typical sample of the regularities normally to be encountered, it seems not unreasonable (as I have argued elsewhere (MacKay, 1955)) that the organizing system should now be able to grow in complexity in the ways we have discussed, step by step with the effector system, at a rate comparable with that of the learning process in human infancy. At least the system may perhaps bear realistic comparison with the human one, if only in order that the differences as they emerge may lead to its refinement.

But at present we are concerned not with the comparison so much as with the fact that this kind of activity is possible in a self-organizing information-flow system, and with its significance as one solution to the problem of concept formation.

For it is indeed in the same principles of organization which we have hitherto treated as a means of improving goal pursuit, that we shall find a possible solution to the problems of inventive activity—particularly concept formation—which are our chief concern.

VI. THE REPRESENTATION OF THE FIELD OF ACTIVITY

(i) *The symbolization of information.* In a digital computor the symbols for concepts are patterns of 1's and 0's or their equivalents such as electrical dots and dashes. In an analogue computor such as a guided missile might use, abstractions such as 'distance of target' are represented by the magnitudes of voltages or currents. What corresponds to these symbols in our present system? How is information about its field of action represented internally?

(ii) *Pre-conceptual information.* At the first stage, the stage of *reception*, it is represented by what we have called the mismatch signal, which may include, for instance, electrical stimuli from visual receptors. Much of this information may be coupled directly to conditional-probability controls associated with actions which in a human being we should describe as reflexes. Incoming information then leads to a conditioned or unconditioned response without any necessary intermediate stage of conceptualization.

In human beings such activities range from visceral responses of which we may be unaware, through such reflexes as swallowing which we can sometimes control, to learned patterns such as swimming or cycling which may have formed under conscious guidance. All are characterized by the fact that we 'don't think about them'. We say we 'know how to' cycle, etc., using 'know' in a different sense from that which we use for conceptualized 'facts'.

(iii) *Symbolization in terms of organizing activity.* But in our information-flow model there is a second sort of internal activity which logically 'represents' features of the world of activity. This is the activity of the internal organizing system. Its imitative sub-routines, when evoked in order to match successfully some change in the world of activity, form an active symbol for that change. What is more, since most sub-routines will tend to form out of standard sequences, this symbol will amount logically to a description in terms of the corresponding standard categories. In normal circumstances, then, there will be in the organizing activity a running representation of those features of the field of action to which the system as a whole is currently matching its own activity, internal and external.

In its 'visual' system, for example, we may picture the internal activity as organized to match (in a sense, to cancel out actively) the incoming visual signals. It will do this as a result of combinations of sub-routines of organizing activity which will have developed through adaptive success in the past, and will amount therefore to a representation of the visual field in terms of previously exemplified categories.

(iv) *The correlate of perception.* Once again the details cannot detain us here. But the essential point is that the symbols constituted by the internally evoked matching response (the organizing activity) seem to be related to the incoming recepta from the sensory organs of the information-flow model in much the same way as what we call *percepts* are related to the recepta from human sensory organs.

Among other points worthy of note, we see that (*a*) the description offered by this symbolic representation is selective: certain features are 'noticed' and others ignored; (*b*) the description possible at a given level is not unique, since several combinations may 'match' equally well; there will be illusions and 'ambiguous figures' for this system just as for human observers (e.g. the Ames demonstrations (Ames, 1946)); (*c*) complementary descriptions may be constituted by organizing activity at different levels of abstraction (e.g. an illuminated advertizing sign may be perceived either as a number of electric lamps or as the sequence of words delineated by them); and (*d*) the state of readiness both for further effector action and for further internal organizing activity depends on the pattern of these symbols, in the same logical way as our human state of readiness depends on what we perceive in our world of activity.

These are only a few of many points in favour of attributing to the organizing sub-routines the role of symbols for percepts.

(v) *Imagination and thinking.* But we must go further. What we know is not limited to what we currently perceive. We know 'what is the case'—or believe we do—even after we have shut our eyes, or gone away for a year from a familiar landscape. Operationally we mean by this, among other things, (*a*) that we can 'evoke mental images' of the landscape, or of the relationship (concrete or abstract) which we claim to know; (*b*) that in all appropriate circumstances we are organized to react to the pattern of events which should come about if 'what we believe' *is* the case. For example, we move so as to avoid the chair which we believe to stand in the middle of a dark room. Or we (consciously or unconsciously) select the statements we make to a friend so as to avoid logical contradiction with a proposition we believe.

Now the model shows certain features of its activity that seem closely parallel to these. In particular, it is possible and would indeed be normal in the absence of excessive external stimulation, for some of the internal organizing mechanisms to 'run free' without evoking the corresponding effector activity. All that is necessary is a flexible inhibitory system under the general control of the organizers, so that they can suppress, in whole or in part, the effector action normally induced by the free running activity.

When this happens, long trains of organizing activity will normally be excited through the statistical couplings formed as a result of past 'experience', and internal goal-guided activity can take place, in which the goal pursued is *abstractly defined in terms of the degrees of freedom of the organizing system*, in the same way as the goal X (Fig. 1) was defined in terms of those of the effector system E. Along such lines it seems possible to find correlates of many of the human activities we group under the names of *thinking* and *imagining*.

Hypotheses, for example, are operationally symbolized by sub-routines which organize readinesses for the corresponding features that should be shown by the world of activity if the hypotheses were true. A familiar scene can be 'imagined' by re-evoking the pattern of internal organizing activity by which it was 'matched' originally. An unfamiliar scene, known only from a description, can be 'imagined' by allowing the internal activity symbolizing its description to evoke what it will by way of activity statistically associated with past instances of the descriptive terms. If the model has come to use organizing routines mainly from its visual system as symbols for concepts, such evoked activity may amount to a symbolization of a complete visual image. If, however, some of the other types of organizing routines form its main conceptual vocabulary, the visual component of its 'imagined' situation may be small. All such 'imaginative' activity is distinguished from 'hallucination' by the fact that the mismatch signal will be present in its normal channels, indicating that what is symbolized is not a representation of the current state of the environment.

(vi) *Functional disorder.* This raises the whole question of functional disorder in the automaton, and its behavioural resemblances to human mental disorder. But the detailed consideration of this topic would lead too far and will be undertaken in further papers. (Some of the operational differences between normal imagination, illusion (e.g. optical), and hallucination, are discussed in an earlier paper (MacKay, 1952).) Suffice it to say that the behaviour accompanying differential threshold changes, and diversion or interruption of the mismatch signal (in various ways), adds up to a picture of which no major feature is without its known correlate in human psychopathology, and in which many of the major symptoms of mental disorder find natural analogues.

VII. CONCLUSION

And so we could go on, but we must bring this lengthy discussion to an end; for the outlines of the correlation possible between the model's internal activity and human concept-handling processes should now be clear. And perhaps enough has been said to indicate that such a self-organizing system shows some promise of usefulness as a kind of foil for psychological theories of human personality. Of each point of debate we may ask 'What difference would it make in the information-flow model?' or 'How does it sound when translated into terms of the information-flow pattern?' Needless to say, the information-flow model is likely to find itself refined as often as the psychological theories in consequence of such discipline; but it seems at least possible that the process may be a convergent one that could materially further psychological understanding.

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IS CONSCIOUSNESS A BRAIN PROCESS?

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The thesis that consciousness is a process in the brain is put forward as a reasonable scientific hypothesis, not to be dismissed on logical grounds alone. The conditions under which two sets of observations are treated as observations of the same process, rather than as observations of two independent correlated processes, are discussed. It is suggested that we can identify consciousness with a given pattern of brain activity, if we can explain the subject's introspective observations by reference to the brain processes with which they are correlated. It is argued that the problem of providing a physiological explanation of introspective observations is made to seem more difficult than it really is by the 'phenomenological fallacy', the mistaken idea that descriptions of the appearances of things are descriptions of the actual state of affairs in a mysterious internal environment.

I. INTRODUCTION

The view that there exists a separate class of events, mental events, which cannot be described in terms of the concepts employed by the physical sciences no longer commands the universal and unquestioning acceptance amongst philosophers and psychologists which it once did. Modern physicalism, however, unlike the materialism of the seventeenth and eighteenth centuries, is behaviouristic. Consciousness on this view is either a special type of behaviour, 'sampling' or 'running-back-and-forth' behaviour as Tolman (1932, p. 206) has it, or a disposition to behave in a certain way, an itch for example being a temporary propensity to scratch. In the case of cognitive concepts like 'knowing', 'believing', 'understanding', 'remembering' and volitional concepts like 'wanting' and 'intending', there can be little doubt, I think, that an analysis in terms of dispositions to behave (Wittgenstein, 1953; Ryle, 1949) is fundamentally sound. On the other hand, there would seem to be an intractable residue of concepts clustering around the notions of consciousness, experience, sensation and mental imagery, where some sort of inner process story is unavoidable (Place, 1954). It is possible, of course, that a satisfactory behaviouristic account of this conceptual residuum will ultimately be found. For our present purposes, however, I shall assume that this cannot be done and that statements about pains and twinges, about how things look, sound and feel, about things dreamed of or pictured in the mind's eye, are statements referring to events and processes which are in some sense private or internal to the individual of whom they are predicated. The question I wish to raise is whether in making this assumption we are inevitably committed to a dualist position in which sensations and mental images form a separate category of processes over and above the physical and physiological processes with which they are known to be correlated. I shall argue that an acceptance of inner processes does not entail dualism and that the thesis that consciousness is a process in the brain cannot be dismissed on logical grounds.

II. THE 'IS' OF DEFINITION AND THE 'IS' OF COMPOSITION

I want to stress from the outset that in defending the thesis that consciousness is a process in the brain, I am not trying to argue that when we describe our dreams, fantasies and sensations we are talking about processes in our brains. That is, I am not claiming that statements about sensations and mental images are reducible to or analysable into statements about brain processes, in the way in which 'cognition statements' are analysable into statements about behaviour. To say that statements about consciousness are statements about brain processes is manifestly false. This is shown (*a*) by the fact that you can describe your sensations and mental imagery without knowing anything about your brain processes or even that such things exist, (*b*) by the fact that statements about one's consciousness and statements about one's brain processes are verified in entirely different ways and (*c*) by the fact that there is nothing self-contradictory about the statement '*X* has a pain but there is nothing going on in his brain'. What I do want to assert, however, is that the statement 'consciousness is a process in the brain', although not necessarily true, is not necessarily false. 'Consciousness is a process in the brain', on my view is neither self-contradictory nor self-evident; it is a reasonable scientific hypothesis, in the way that the statement 'lightning is a motion of electric charges' is a reasonable scientific hypothesis.

The all but universally accepted view that an assertion of identity between consciousness and brain processes can be ruled out on logical grounds alone, derives, I suspect, from a failure to distinguish between what we may call the 'is' of definition and the 'is' of composition. The distinction I have in mind here is the difference between the function of the word 'is' in statements like 'a square is an equilateral rectangle', 'red is a colour', 'to understand an instruction is to be able to act appropriately under the appropriate circumstances', and its function in statements like 'his table is an old packing case', 'her hat is a bundle of straw tied together with string', 'a cloud is a mass of water droplets or other particles in suspension'. These two types of 'is' statement have one thing in common. In both cases it makes sense to add the qualification 'and nothing else'. In this they differ from those statements in which the 'is' is an 'is' of predication; the statements 'Toby is 80 years old and nothing else', 'her hat is red and nothing else' or 'giraffes are tall and nothing else', for example, are nonsense. This logical feature may be described by saying that in both cases both the grammatical subject and the grammatical predicate are expressions which provide an adequate characterization of the state of affairs to which they both refer.

In another respect, however, the two groups of statements are strikingly different. Statements like 'a square is an equilateral rectangle' are necessary statements which are true by definition. Statements like 'his table is an old packing case', on the other hand, are contingent statements which have to be verified by observation. In the case of statements like 'a square is an equilateral rectangle' or 'red is a colour', there is a relationship between the meaning of the expression forming the grammatical predicate and the meaning of the expression forming the grammatical subject, such that whenever the subject expression is applicable the predicate must also be applicable. If you can describe something as red then you must also be able to describe it as coloured. In the case of statements like 'his table is an old packing case', on the other hand, there is no such relationship between the meanings of the expressions 'his table' and 'old packing case'; it merely so happens that in this case both expressions are applicable to and at the same time provide an adequate characterization of the same object. Those who contend that the statement 'consciousness is a brain process' is logically untenable base their claim, I suspect, on the mistaken assumption that if the meanings of two statements or expressions are quite unconnected, they cannot both provide an adequate characterization of the same object or state of affairs: if something is a state of consciousness, it cannot be a brain process, since there is nothing self-contradictory in supposing that someone feels a pain when there is nothing happening inside his skull. By the same token we might be led to conclude that a table cannot be an old packing case, since there is nothing self-contradictory in supposing that someone has a table, but is not in possession of an old packing case.

III. THE LOGICAL INDEPENDENCE OF EXPRESSIONS AND THE ONTOLOGICAL INDEPENDENCE OF ENTITIES

There is, of course, an important difference between the table/packing case case and the consciousness/brain process case in that the statement 'his table is an old packing case' is a particular proposition which refers only to one particular case, whereas the statement 'consciousness is a process in the brain' is a general or universal proposition applying to all states of consciousness whatever. It is fairly clear, I think, that if we lived in a world in which all tables without exception were packing cases, the concepts of 'table' and 'packing case' in our language would not have their present logically independent status. In such a world a table would be a species of packing case in much the same way that red is a species of colour. It seems to be a rule of language that whenever a given variety of object or state of affairs has two characteristics or sets of characteristics, one of which is unique to the variety of object or state of affairs in question, the expression used to refer to the characteristic or set of characteristics which defines the variety of object or state of affairs in question will always entail the expression used to refer to the other characteristic or set of characteristics. If this rule admitted of no exception it would follow that any expression which is logically independent of another expression which uniquely characterizes a given variety of object or state of affairs, must refer to a characteristic or set of characteristics which is not normally or necessarily associated with the object or state of affairs in question. It is because this rule applies almost universally, I suggest, that we are normally justified in arguing from the logical independence of two expressions to the ontological independence of the states of affairs to which they refer. This would explain both the undoubted force of the argument that consciousness and brain processes must be independent entities because the expressions used to refer to them are logically independent and, in general, the curious phenomenon whereby questions about the furniture of the universe are often fought and not infrequently decided merely on a point of logic.

The argument from the logical independence of two expressions to the ontological independence of the entities to which they refer breaks down in the case of brain processes and consciousness, I believe, because this is one of a relatively small number of cases where the rule stated above does not apply. These exceptions are to be found, I suggest, in those cases where the operations which have to be performed in order to verify the presence of the two sets of characteristics inhering in the object or state of affairs in question can seldom if ever be performed simultaneously. A good example here is the case of the cloud and the mass of droplets or other particles in suspension. A cloud is a large semi-transparent mass with a fleecy texture suspended in the atmosphere whose shape is subject to continual and kaleidoscopic change. When observed at close quarters, however, it is found to consist of a mass of tiny particles, usually water droplets, in continuous motion. On the basis of this second observation we conclude that a cloud is a mass of tiny particles and nothing else. But there is no logical connexion in our language between a cloud and a mass of tiny particles; there is nothing self-contradictory in talking about a cloud which is not composed of tiny particles in suspension. There is no contradiction involved in supposing that clouds consist of a dense mass of fibrous tissue; indeed, such a consistency seems to be implied by many of the functions performed by clouds in fairy stories and mythology. It is clear from this that the terms 'cloud' and 'mass of tiny particles in suspension' mean quite different things. Yet we do not conclude from this that there must be two things, the mass of particles in suspension and the cloud. The reason for this, I suggest, is that although the characteristics of being a cloud and being a mass of tiny particles in suspension are invariably associated, we never make the observations necessary to verify the statement 'that is a cloud' and those necessary to verify the statement 'this is a mass of tiny particles in suspension' at one and the same time. We can observe the micro-structure of a cloud only when we are enveloped by it, a condition which effectively prevents us from observing those characteristics which from a distance lead us to describe it as a cloud. Indeed, so disparate are these two experiences that we use different words to describe them. That which is a cloud when we observe it from a distance becomes a fog or mist when we are enveloped by it.

IV. WHEN ARE TWO SETS OF OBSERVATIONS OBSERVATIONS OF THE SAME EVENT?

The example of the cloud and the mass of tiny particles in suspension was chosen because it is one of the few cases of a general proposition involving what I have called the 'is' of composition which does not involve us in scientific technicalities. It is useful because it brings out the connexion between the ordinary everyday cases of the 'is' of composition like the table/packing case example and the more technical cases like 'lightning is a motion of electric charges' where the analogy with the consciousness/brain process case is most marked. The limitation of the cloud/tiny particles in suspension case is that it does not bring out sufficiently clearly the crucial problem of how the identity of the states of affairs referred to by the two expressions is established. In the cloud case the fact that something is a cloud and the fact that something is a mass of tiny particles in suspension are both verified by the normal processes of visual observation. It is arguable, moreoever, that the identity of the entities referred to by the two expressions is established by the continuity between the two sets of observations as the observer moves towards or away from the cloud. In the case of brain processes and consciousness there is no such continuity between the two sets of observations involved. A closer introspective scrutiny will never reveal the passage of nerve impulses over a thousand synapses in the way that a closer scrutiny of a cloud will reveal a mass of tiny particles in suspension. The operations required to verify statements about consciousness and statements about brain processes are fundamentally different.

To find a parallel for this feature we must examine other cases where an identity is asserted between something whose occurrence is verified by the ordinary processes of observation and something whose occurrence is established by special scientific procedures. For this purpose I have chosen the case where we say that lightning is a motion of electric charges. As in the case of consciousness, however closely we scrutinize the lightning we shall never be able to observe the electric charges, and just as the operations for determining the nature of one's state of consciousness are radically different from those involved in determining the nature of one's brain processes, so the operations for determining the occurrence of lightning are radically different from those involved in determining the occurrence of a motion of electric charges. What is it, therefore, that leads us to say that the two sets of observations are observations of the same event? It cannot be merely the fact that the two sets of observations are systematically correlated such that whenever there is lightning there is always a motion of electric charges. There are innumerable cases of such correlations where we have no temptation to say that the two sets of observations are observations of the same event. There is a systematic correlation, for example, between the movement of the tides and the stages of the moon, but this does not lead us to say that records of tidal levels are records of the moon's stages or vice versa. We speak rather of a causal connexion between two independent events or processes.

The answer here seems to be that we treat the two sets of observations as observations of the same event, in those cases where the technical scientific observations set in the context of the appropriate body of scientific theory provide an immediate explanation of the observations made by the man in the street. Thus we conclude that lightning is nothing more than a motion of electric charges, because we know that a motion of electric charges through the atmosphere, such as occurs when lightning is reported, gives rise to the type of visual stimulation which would lead an observer to report a flash of lightning. In the moon/tide case, on the other hand, there is no such direct causal connexion between the stages of the moon and the observations made by the man who measures the height of the tide. The causal connexion is between the moon and the tides, not between the moon and the measurement of the tides.

V. THE PHYSIOLOGICAL EXPLANATION OF INTROSPECTION AND THE PHENOMENOLOGICAL FALLACY

If this account is correct, it should follow that in order to establish the identity of consciousness and certain processes in the brain, it would be necessary to show that the introspective observations reported by the subject can be accounted for in terms of processes which are known to have occurred in his brain. In the light of this suggestion it is extremely interesting to find that when a physiologist as distinct from a philosopher finds it difficult to see how consciousness could be a process in the brain, what worries him is not any supposed self-contradiction involved in such an assumption, but the apparent impossibility of accounting for the reports given by the subject of his conscious processes in terms of the known properties of the central nervous system. Sir Charles Sherrington has posed the problem as follows: 'The chain of events stretching from the sun's radiation entering the eye to, on the one hand, the contraction of the pupillary muscles, and on the other, to the electrical disturbances in the brain-cortex are all straightforward steps in a sequence of physical "causation", such as, thanks to science, are intelligible. But in the second serial chain there follows on, or attends, the stage of brain-cortex reaction an event or set of events quite inexplicable to us, which both as to themselves and as to the causal tie between them and what preceded them science does not help us; a set of events seemingly incommensurable with any of the events leading up to it. The self "sees" the sun; it senses a two-dimensional disc of brightness, located in the "sky", this last a field of lesser brightness, and overhead shaped as a rather flattened dome, coping the self and a hundred other visual things as well. Of hint that this is within the head there is none. Vision is saturated with this strange property called "projection", the unargued inference that what it sees is at a "distance" from the seeing "self". Enough has been said to stress that in the sequence of events a step is reached where a physical situation in the brain leads to a psychical, which however contains no hint of the brain or any other bodily part.... The supposition has to be, it would seem, two continuous series of events, one physico-chemical, the other psychical, and at times interaction between them' (Sherrington, 1947, pp. xx-xxi).

Just as the physiologist is not likely to be impressed by the philosopher's contention that there is some self-contradiction involved in supposing consciousness to be a brain process, so the philosopher is unlikely to be impressed by the considerations which lead Sherrington to conclude that there are two sets of events, one physico-chemical, the other psychical. Sherrington's argument for all its emotional appeal depends on a fairly simple logical mistake, which is unfortunately all too frequently made by psychologists and physiologists and not infrequently in the past by the philosophers themselves. This logical mistake, which I shall refer to as the 'phenomenological fallacy', is the mistake of supposing that when the subject describes his experience, when he describes how things look, sound, smell, taste or feel to him, he is describing the literal properties of objects and events on a peculiar sort of internal cinema or television screen, usually referred to in the modern psychological literature as the 'phenomenal field'. If we assume, for example, that when a subject reports a green after-image he is asserting the occurrence inside himself of an object which is literally green, it is clear that we have on our hands an entity for which there is no place in the world of physics. In the case of the green after-image there is no green object in the subject's environment corresponding to the description that he gives. Nor is there anything green in his brain; certainly there is nothing which could have emerged when he reported the appearance of the green after-image. Brain processes are not the sort of things to which colour concepts can be properly applied.

The phenomenological fallacy on which this argument is based depends on the mistaken assumption that because our ability to describe things in our environment depends on our consciousness of them, our descriptions of things are primarily descriptions of our conscious experience and only secondarily, indirectly and inferentially descriptions of the objects and events in our environments. It is assumed that because we recognize things in our environment by their look, sound, smell, taste and feel, we begin by describing their phenomenal properties, i.e. the properties of the looks, sounds, smells, tastes and feels which they produce in us, and infer their real properties from their phenomenal properties. In fact, the reverse is the case. We begin by learning to recognize the real properties of things in our environment. We learn to recognize them, of course, by their look, sound, smell, taste and feel; but this does not mean that we have to learn to describe the look, sound, smell, taste and feel of things before we can describe the things themselves. Indeed, it is only after we have learnt to describe the things in our environment that we can learn to describe our consciousness of them. We describe our conscious experience not in terms of the mythological 'phenomenal properties' which are supposed to inhere in the mythological 'objects' in the mythological 'phenomenal field', but by reference to the actual physical properties of the concrete physical objects, events and processes which normally, though not perhaps in the present instance, give rise to the sort of conscious experience which we are trying to describe. In other words when we describe the after-image as green, we are not saying that there is something, the after-image, which is green, we are saying that we are having the sort of experience which we normally have when, and which we have learnt to describe as, looking at a green patch of light.

Once we rid ourselves of the phenomenological fallacy we realize that the problem of explaining introspective observations in terms of brain processes is far from insuperable. We realize that there is nothing that the introspecting subject says about his conscious experiences which is inconsistent with anything the physiologist might want to say about the brain processes which cause him to describe the environment and his consciousness of that environment in the way he does. When the subject describes his experience by saying that a light which is in fact stationary, appears to move, all the physiologist or physiological psychologist has to do in order to explain the subject's introspective observations, is to show that the brain process which is causing the subject to describe his experience in this way, is the sort of process which normally occurs when he is observing an actual moving object and which therefore normally causes him to report the movement of an object in his environment. Once the mechanism whereby the individual describes what is going on in his environment has been worked out, all that is required to explain the individual's capacity to make introspective observations is an explanation of his ability to discriminate between those cases where his normal habits of verbal description are appropriate to the stimulus situation and those cases where they are not and an explanation of how and why, in those cases where the appropriateness of his normal descriptive habits is in doubt, he learns to issue his ordinary descriptive protocols preceded by a qualificatory phrase like 'it appears', 'seems', 'looks', 'feels', etc.

I am greatly indebted to my fellow-participants in a series of informal discussions on this topic which took place in the Department of Philosophy, University of Adelaide, in particular to Mr C. B. Martin for his persistent and searching criticism of my earlier attempts to defend the thesis that consciousness is a brain process, to Prof. D. A. T. Gasking, of the University of Melbourne, for clarifying many of the logical issues involved and to Prof. J. J. C. Smart for moral support and encouragement in what often seemed a lost cause.

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LISTENING BETWEEN AND DURING PRACTISED AUDITORY DISTRACTIONS

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Subjects were instructed to answer a rapid series of questions, and during the series a buzzer was sometimes sounded. Some subjects were instructed to make a reaction to this buzzer; this produced an interference with the speech task, both at the instant of the buzz and also between buzzes. The latter interference was reduced by previous practice at the buzzer reaction, but the former was not.

If the buzzer was one of two possible signals for a two-choice reaction, the interference at the moment of the buzz was greater, suggesting that the situation is most easily described in the terms of information theory. But previous practice in this case causes the subject to perform the secondary task in a way which requires less information and so interferes less. The effect of practice in 'automatizing' a task is thus real, but has important limitations.

I. Introduction

Human performance is often said to change with practice by becoming more 'automatic'. By this is meant, on the one hand, that a practised response is less likely to give rise to introspective reports; as was found, for example, by Solomons (1899) and Bryan & Harter (1897). On the other hand, it is usually said that an automatized habit interferes less with other simultaneous activity (James, 1890). These two characteristics may be essentially the same, since introspective report and bodily action can from one point of view be taken as different responses to the same stimulus. When both appear, the activity within the organism is clearly more widespread than when introspective report is lacking. Such a reduction in the number of responses to one stimulus may well involve an increased ability to make simultaneous responses to different stimuli. The present paper is therefore intended to examine the extent to which practice on one task may allow other tasks to be carried out at the same time.

The particular tasks used all involve hearing, since previous work with that sense (Broadbent, 1952) has shown interference, between tasks, which cannot be ascribed to difficulties of sensory adjustment. Results from the ear can therefore be extended to, say, the eye more confidently than in the reverse direction, though such an extension must be tentative until further evidence is available. The earlier work has also shown practice effects in the recognition of heard voices in a babel, which makes it desirable to use a similar technique when investigating practice more thoroughly. Furthermore, it is especially true of auditory tasks that everyday experience places great emphasis on the value of practice: those who operate communication systems in which several voices may be heard at once find that continued familiarity with the situation brings impressive subjective benefits. Johnston (1953) has recently pointed out that workers in a noisy factory may understand speech in that factory though not in other noisy situations.

II. Method

The plan was adopted of asking all subjects to carry out a task involving speech perception, on which a standard amount of practice was given. Additional tasks were then added to the central one, the nature and degree of familiarity of the additional task varying with different groups of subjects; and the amount of interference produced in the central task was determined.

The central task was that used by Broadbent (1952). The subject observed a piece of paper marked in five divisions, each section carrying two abstract symbols (Fig. 1). The paper was changed from time to time. He heard messages consisting of the names of two symbols and the number of a position; if those two symbols agreed with the two shown in that position he said 'yes', and if not, 'no'. Each message was also prefixed by one of two possible call signs, and the particular one used had to be repeated by the subject before his answer. These messages were delivered through a Grampian G. 129 loudspeaker mounted in front of the subject 3 ft. away and slightly above head-level, the speaker being fed from one channel of a Ferrograph two-channel tape recorder. All subjects received a standard practice period of 10 min. on this task on the day before the main experiment.

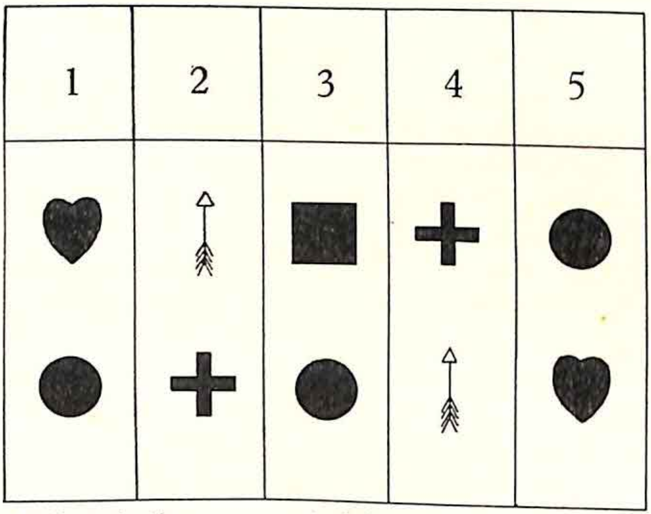

Fig. 1. The visual part of the task; the arrangement of the symbols was changed at every twelve messages. A typical message would be 'GDO Square Circle Three?' The appropriate answer would be 'GDO Yes', with the symbols as shown. See text for further explanation.

On the experimental day all subjects heard four runs, each of twenty-four questions separated by 2 sec. intervals so that each run lasted for approximately 1·3 min. This is a rather fast rate, resulting in an appreciable number of errors. A second loudspeaker, built into the Ferrograph case, was mounted on the subject's left-hand side 3 ft. away at shoulder height. This was fed from the second channel of the recorder, and as every fifth question was spoken through the central speaker, a buzzer sounded through the left-hand speaker, starting with the second word of the question and finishing at the end of the third. Thus this occurred four times in each run. The output levels of both channels were kept constant throughout the experiment; each gave 80 db. on a General Radio sound-level meter at the subject's position. The order in which the four records were heard was rotated within each group of subjects. All ninety-two subjects were naval ratings aged 30 or less. The majority were Fleet Air Arm ratings; in certain groups a few ratings of other trades were included, but a check on the scores obtained on the speech task in the standard practice session showed that this did not affect the comparability of the groups. No subject served in more than one group. There were eight groups in all, and of these the first three were the most important, i.e. groups NR, K and KP.

Each group had either ten or twelve subjects. The instructions to each group were as follows. Each group is identified by letters standing for the type of instructions received (see also Table 1):

Group NR (No Response) were instructed that a buzzer would sound from time to time, but that it was to be ignored.

Group K (Key) were instructed that when the buzzer sounded they were to press a key provided, 'without letting it interfere with answering the messages'.

Group KP (Key after Practice) received the same instructions as group K. On the previous day, however, they had heard a record of twenty buzzes similar to those used on the experimental day, separated on the average by 3 sec., and had been instructed to press the key whenever the buzzer sounded. No messages were presented while this practice was going on. On the experimental day another similar practice session immediately preceded the main session.

Group NR–K (No Response, then Key) received the same instructions as group NR on the first two runs, but the same as group K on the last two runs.

Group S–K (Stamp, then Key) received the same instructions as group K, save that the response required was a stamp of the foot during the first two runs. On the last two runs this was changed to the usual pressing of a key.

Group LP–K (Listening Practice, then Key) were treated similarly to group KP except for the content of their practice sessions. In each session they carried out a task in which two numbers were heard simultaneously on the two speakers. One of these numbers was always 1, although it might be on either speaker, and the task was to repeat the other number. Forty pairs of numbers were given in each session, the interval between pairs being 2 sec. This task was intended to give practice at selective listening without requiring the actual stimuli and responses used in the other tasks.

Group K/S (Key or Stamp) were instructed that when the buzzer sounded they were to press the key, provided the experimenter remained inactive. But if the experimenter laid his hand over the key as the buzzer sounded the subject was to stamp his foot instead. (The key was for all groups placed next to the visual display (Fig. 1) so as to be within the field of view; and for all groups the experimenter sat next to the key. But he remained inactive except in this group and group K/SP.) Two such extra stimuli were given in each run, allotted unsystematically between the four buzzes, although given at the same points to all subjects in the group.

Group K/SP (Key or Stamp after Practice) received the same instructions as group K/S but also had two practice sessions as in group KP. In each session half the buzzes at random were accompanied by the extra hand stimulus, and on these occasions the subject had to stamp his foot instead. Thus, like group KP, they practised the reaction actually required in the main session.

In all groups the subjects carried out the response to the buzzer as instructed; it appeared that they treated this task rather than the listening one as the most important.

III. ANALYSIS OF DATA

All scores used were in terms of errors in answering the questions. Two main classes of score may be distinguished: first, errors on questions which arrived at the same time as the buzzer sounded. These will be termed *buzz errors*. Secondly, errors on the questions arriving between the times when the buzzer sounded. These will be termed *between-buzz errors*. In the latter class the questions immediately before and after buzzes furnish a few interesting comparisons, and will be referred to as *before* (or *after*) *buzz errors*.

As by the very nature of the problem only a few readings could be obtained from each subject on buzz errors, analysis of variance could not be used. Even in the more numerous between-buzz errors, Bartlett's test showed significant non-homogeneity of variance. Adequate tests were, however, devised as follows.

For all comparisons between different groups of subjects the tau technique of Whitfield (1947) was employed. For comparisons between two different scores of the same subject, such as practice effects, tau was inappropriate. In such cases the total score for each subject, including both conditions, was calculated and expressed as a proportion of the total possible. On the null hypothesis of zero difference between conditions, if

$$\text{this proportion} = p,$$
$$q = 1 - p,$$
$$\text{number of questions under each condition} = n,$$

then $2npq$ gives the variance of the difference between conditions for that subject. The variance of the total difference for all subjects is of course equal to the sum of the individual variances, and the total difference was tested against it; the 0·05 level requires a critical ratio of 2, and the 0·01 level one of 2·6.

The critical ratios for the comparisons mentioned in the results are given in Table 2. Wherever a ratio is accepted as significant with a value between 2·0 and 2·1 (that is, in border-line cases), analysis of variance was also carried out. For this analysis when the number of possible errors under each condition was eight or less, each score x was transformed to $\sin^{-1}\sqrt{x}$, the usual transformation for data of this type. The values of t from analysis of variance are given in brackets in Table 2 for these cases; they are more questionable than the methods already described, since the assumptions of analysis of variance are not valid here, but they provide some assurance that more familiar methods do not give wildly different results. One comparison in group K/S and one in group NR–K are the most doubtful and are considered in more detail in the section on results.

One comparison, that of buzz and between-buzz errors for group NR, was so marked that every subject in the group showed the effect. This result was therefore treated as significant from binomial theorem without any further test.

IV. RESULTS

Effects of an irrelevant stimulus. Group NR acts as a control for the other groups, since these subjects had to carry out the task without reacting to the buzzer. It is notable that even so they made more mistakes when the buzzer sounded; buzz errors were more frequent than between-buzz errors, $p < 0.001$. Part of this interference may be due to cochlear masking, as the signal-noise ratio is only unity.

Errors at the instant when another reaction is required. The major feature of these results is this: when an extra signal occurs during listening to speech, and this signal is not to be ignored but to produce some action from the listener, the speech is impaired both at the instant of this extra response and also between responses. Previous practice at the extra reaction helps to reduce the errors between but not during the signals for these extra reactions. The evidence for these statements is as follows.

The interference at the instant of response is shown by the fact that group K shows more buzz errors than group NR, $p < 0.05$. But this interference is not reduced by previous practice at key-pressing to the buzzer, by previous practice at selective listening, or by change in the reaction required from foot-stamping to key-pressing; groups KP, LP–K, and S–K all showed scores for buzz errors closely similar to those of group K (critical ratio < 1 in each case).

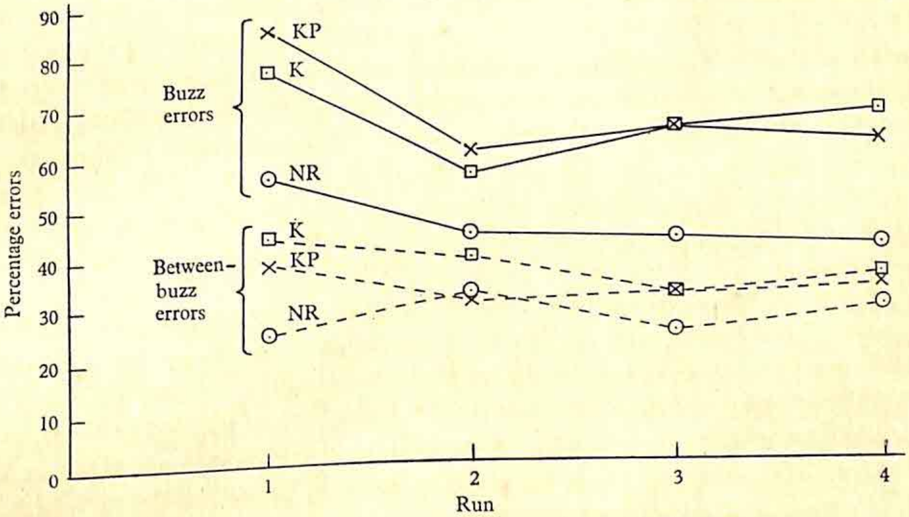

Fig. 2. The performance of the three most important groups.

Table 1. *Percentage of errors under each condition*

No. of subjects	Group	Conditions	Buzz errors				Between-buzz errors			
			Run 1	Run 2	Run 3	Run 4	Run 1	Run 2	Run 3	Run 4
12	NR	Ignore buzz	56	44	44	42	25	32	25	30
12	K	Press key to buzz	77	56	65	69	42	40	33	37
10	KP	Press key, previous practice at key-pressing	85	60	65	62	39	31	33	35
12	NR–K	Runs 1 and 2, ignore buzz. Runs 3 and 4, press key	63	35	83	69	40	35	34 +	39 +
12	S–K	Runs 1 and 2, stamp foot. Runs 3 and 4, press key	88	67	60	69	45	41	41	35
12	LP–K	Press key, previous practice at selective listening	79	63	54	71	45	50	43	43
10	K/S	Press key, or stamp foot if visual signal with buzz	92	78	77	80	38	38	29 +	27 +
12	K/SP	As K/S, previous practice at these reactions	85	71	71	73	33 +	33 +	30 +	26 +

For significance see text. Entries marked + exclude after-buzz errors and so are not directly comparable to the others given.

Group K/S was required to make a two-choice reaction when the buzzer sounded. In the first half of the session they showed more buzz errors than group K, the corresponding simple reaction group, $p < 0.05$. In the second half of the session, however, the subjects began to treat the situation as effectively one-choice, starting to press the key to every buzz and correcting the response later if incorrect (see data given in the section on controls below). Correspondingly the number of buzz errors ceased to be significantly greater than in group K (C.R. = 1·53). A similar type of reaction was adopted even in the first half of the session by group K/SP, who had been practised previously; and equally their buzz errors were not significantly different from those of group KP (C.R. < 1).

Table 2. *Critical ratios*

Comparisons between different groups (tau tests)				Comparisons within same group (test on variance of proportions)			
Type of score	Groups compared		C.R.	Type of score compared		Group	C.R.
Buzz	K	NR	2·41	Buzz run 1	Buzz run 2	K	2·93
Buzz	K	KP	<1	Buzz run 1	Buzz run 2	KP	2·95
Buzz	K	LP–K	<1	Buzz run 1	Buzz run 2	LP–K	2·18
Buzz	K	S–K	<1	Buzz run 1	Buzz run 2	S–K	2·72
Buzz (first half of session)	K/S	K	2·21	Buzz run 1	Buzz run 2	NR	1·37
Buzz (second half of session)	K/S	K	1·53	Buzz run 1	Buzz run 2	NR–K	3·06
Buzz	K/SP	KP	<1	Buzz run 3	Buzz run 4	NR–K	2·05 (1·90, d.f. 11)
Buzz (second half of session)	NR	K	2·80	Before buzz	After buzz	K	<1
Buzz (second half of session)	NR	S–K	1·91	Before buzz	After buzz	LP–K	<1
Buzz (second half of session)	NR	LP–K	1·92	Before buzz	After buzz	S–K	<1
Buzz (second half of session)	NR	KP	1·79	Before buzz	After buzz	NR	<1
Buzz run 3	NR–K	S–K	2·12	Before buzz	After buzz	KP	<1
Between buzz (first half of session only)	NR	K	2·47	Before buzz (first half of session)	After buzz (first half of session)	K/S	<1
Between buzz (first half of session)	NR	S–K	2·07 (2·21, d.f. 22)	Before buzz	After buzz	K/SP	2·43
Between buzz (first half of session)	NR	LP–K	2·05 (2·35, d.f. 22)	Before buzz (second half of session)	After buzz (second half of session)	K/S	2·97
Between buzz (first half of session)	NR	KP	1·06	Before buzz (second half of session)	After buzz (second half of session)	NR–K	2·02 (1·75, d.f. 11)
Between buzz (first half of session)	KP	K	2·18	Key-pressing questions	Foot-stamping questions	K/S (first half)	<1
Between buzz (first half of session)	NR	K/S	2·12	Key-pressing questions	Foot-stamping questions	K/S (second half)	2·02 (1·65, d.f. 9)
Between buzz (first half of session)	NR	K/SP	<1	Key-pressing questions	Foot-stamping questions	K (second half)	<1
				Key-pressing questions	Foot-stamping questions	K/SP (first half)	3·16

Values in brackets are those for *t* tests on the border-line cases. They are for comparison only, as the assumptions of analysis of variance are not justified in this data.

Thus in all these cases where the subjects are making only the simple choice whether to react or not, buzz errors are the same; although when the subjects have also to choose between two reactions, more buzz errors are found.

Errors between the extra reactions. The interference during the time between reactions appears in between-buzz errors; the comparisons of major interest are for the first half of the session, and the values of p given are for this half only. Groups K, S–K and LP–K, who made simple reactions without previously practising them, gave more between-buzz errors than group NR, $p < 0\cdot05$ in each case. Group KP, who had practised key-pressing, gave no significant difference (C.R. $= 1\cdot06$). This group was significantly better than group K, $p < 0\cdot05$.

In the two complex reaction groups, the unpractised group K/S gave more between-buzz errors than group NR, $p < 0\cdot05$. The practised group K/SP did not (C.R. < 1).

In all unpractised cases then, there is a rise in between-buzz errors caused by requiring a reaction to the buzz, this rise failing to appear after previous practice on the required response. It thus represents a type of interference distinct from that shown by buzz errors.

It remains to note that no group was significantly different from group NR in the second half of the session, though the C.R.'s were usually over 1, and the absence of significance may be due to the low sensitivity of the statistical techniques.

Controls necessary for the above conclusions. Further evidence on points of detail is needed to support the statements already made, and is presented in this section to avoid confusing the reader. The first point requiring more elucidation is the difference between before- and after-buzz errors. This is important because the interference shown by between-buzz errors might have been the spurious result of slow responses to the buzzer interfering with the questions heard immediately after the buzzer. But if this were so, after-buzz errors would be greater than before-buzz errors, and this is not true. For all simple reaction groups except group NR–K, which will be considered separately, the differences were very small and of variable sign, all C.R.'s < 1. The between-buzz interference is not, therefore, due to slow reactions.

By a similar analysis we can detect the point at which the complex reaction groups, K/S and K/SP, begin to treat their task as a simple reaction. Group K/S gives before- and after-buzz errors equal, C.R. < 1 for the first half of the session. In the second half, however, the method of performance has clearly changed, since after-buzz errors are more frequent than before-buzz ones, $p < 0\cdot01$. At the same time there is some evidence that key-pressing responses begin to give less buzz errors than foot-stamping ones, $p < 0\cdot05$, by the proportion test though not by analysis of variance. The latter difference did not appear in the corresponding questions for group K nor in the first half of the session for group K/S. This harmonizes with the data already given and suggests that this group were, by the second half of the session, treating key-pressing as more likely and correcting the response if foot-stamping was required. The connexion between high after-buzz errors and preferential treatment for key-pressing is supported by group K/SP, which shows the same pattern in the first half of the session and more significantly. Key-pressing gave less errors than foot-stamping, $p < 0\cdot01$, and after-buzz errors were more numerous than before-buzz errors, $p < 0\cdot05$.

It is also desirable to know how far exposure to the situation without a response being required may produce the same effects as practice, and this is the function of group NR–K. In the second half of the session, when called upon to react to the buzzer it had previously ignored, this group showed an increase in buzz errors not only to the level of the simple reaction groups, but beyond it, and returned to that level only after another run. This improvement is just significant, $p < 0\cdot05$.

A more satisfactory test is the significant difference in buzz errors on run 3 between this group and group S–K, who changed from one reaction to another. Group NR–K is also the only one of the simple reaction groups to show a marked excess of after-buzz errors over before-buzz ones. The significance is border-line, reaching the 0·05 level by the proportion test but not by analysis of variance; but it may indicate an undue frequency of slow reactions or after-effects of response in this group. The complete absence of both effects in group S–K suggests that previous exposure to a stimulus without reaction may sometimes be more serious in its effects than practice of a different reaction. This has been predicted previously on the basis of other experiments (Broadbent, 1953). It should be noted that higher levels of practice on one response will presumably give the usual negative transfer effect on changing response.

Finally, we should note the effects of practice, not on one reaction by itself, but on the combined tasks. There is a decline in buzz errors after the first run in all the simple reaction groups, $p < 0{\cdot}05$ in each case. (The complex reaction groups are complicated by the known change in their mode of performance.) But a similar though insignificant decline appears in group NR, which ignored the buzzer, and becomes significant, $p < 0{\cdot}01$, in group NR–K. It may be due to compensation for masking effects. The important comparison, therefore, is between the buzz errors of group NR and group K for the second half of the session, and for this difference $p < 0{\cdot}01$. For each of the other groups taken alone $p < 0{\cdot}05$ by a one-tail test. Within the limits of this experiment, then, practice on the combined tasks does not make it as easy to do the two tasks as to do one.

V. Discussion

(*a*) *Simultaneous distraction is not greatly helped by previous practice.* The main finding of these results is clearly the division of the interference between two tasks into two parts. One part arises when two stimuli are presented for response simultaneously. It increases when a reaction is required as opposed to no reaction, and is increased even more by a two-choice reaction (when the latter is performed as such and not as one-choice plus correction). On the other hand, it is more or less unaffected by experiences previous to combining the two tasks. We may point out the relevance of certain concepts of communication theory. In that theory the 'information' contained in a signal is determined not by the intrinsic nature of the signal, but by the probability of its occurrence; in many cases, by the number of alternative signals which might have occurred but did not. Similarly, in these experiments the central task is not impaired merely by the occurrence of an extra event, but by the number of possible situations which might have occurred and from which the actual one must be discriminated. Now any system will have a limit to the amount of information which it can transmit in a given time, this limit being known as the capacity of the channel. It seems fair to describe the interference shown by buzz errors as due to the limited capacity of the human perceptual system, though such a description is of course only justifiable because it says more than the simple statement 'the man is doing too much at once'. The simple statement obscures the vital fact that interference is produced not only by what the man is doing but what he might have to do. This appears, therefore, to be a case in which the language of communication theory shows real advantages.

Such a view also implies that further degrees of practice would be unlikely to show different results, a possibility which no actual experiment can ever rule out. If the perceptual system is of limited capacity, practice on one of two tasks can only be effective by reducing the amount of information analysed at the time of the performance of the task (group K/SP). Some such reduction usually occurs with practice, so that a skill may become more and more 'closed' and independent of outside events (Poulton, 1950); but there is likely to be some limit beyond which this cannot be done. It is noteworthy that the time taken for certain tasks may similarly be reduced down to a certain point which again depends on the minimum quantity of information needing to be gained (Crossman, 1953). In the latter case exact quantification has been employed, but it is not at present possible to specify the informational load demanded by most practical tasks. But this analysis suggests that practice *will not usually be able to abolish completely an interference between two tasks when the stimuli for them arrive simultaneously*. Equally Conrad (1954) has shown that in a complex task signals are more likely to be missed completely by the subject if they occur close in time to a response to some other signal.

(*b*) *Distraction between signals is reduced by practice.* The other part of the interference, however, is obviously reduced by practice. This is an interference with one task arising between the occasions when stimuli from the other task appear. It is apparent that the reduction of this interference will allow two practised tasks to be carried on at the same time, provided critical signals do not arrive simultaneously. This is the equivalent of the classical 'rapid alternation of attention', and appears to explain adequately why those who use multiple channel systems in practical situations place such emphasis on experience.

When a task is 'automatized' by practice, we may therefore say that two kinds of change are taking place. Although the task still appears the same, the person carrying it out now requires less sensory information for successful performance, and also the task ceases to make any demand on him during intervals in its execution. These changes bear a close resemblance to the two features of automatization which were noted in the Introduction, though the change in phrasing has some advantages. We have already noted the parallel between 'interference of two activities' and 'limited capacity of the perceptual system'; and have also noted the reasons for preferring the latter language. The parallel between reduced introspective report and the ability to deal with stimuli alternating from two tasks is perhaps less clear. Yet an interference with one task between successive stimuli from another task must mean that some activity connected with those stimuli is persisting between them, that they are in a sense related to one another and to other intervening events. Introspective accounts of reaction would equally appear to demand relation of the particular stimulus to the whole sequence arriving at the individual. Since both functions also cease to appear with repeated practice, it is tempting to postulate that they are identical.

It is interesting to note that a few other experiments using rather different types of situation support these general conclusions; data drawn from this task alone are naturally a dangerous ground for generalization. Gregg & Brogden (1952) showed that thresholds in one sense obtained with simultaneous stimulation through another were higher if both stimuli were to be reported, and actually lower if only one was to be reported. This corresponds to the first type of interference due to informational load. Another related experiment is that of Baker, Wylie & Gagné (1951) on the effects of interference in motor skill, using various degrees of practice on the task which was to be scored. They confirm that interference continues to exist up to the highest level of practice used, which we have already stated as a consequence of regarding part of the interference between two tasks as due to informational load. Bahrick, Noble & Fitts (1954) showed that interference between two tasks is reduced with practice when the stimuli for one of them arrive in a predictable sequence, but not when the stimuli arrive in an irregular sequence and thus continue to convey information.

Interference with one task when stimuli for another were not present was shown by Mackworth (1950); his subjects did worse on the Clock Test when told that a telephone might ring. But practice effects were not studied in this case. Baker *et al.* (1951) showed that when a new task was added to a motor skill at various levels of practice, there was always an extra temporary interference which died away until all groups showed equal amounts of interference. This temporary decrement may correspond to the between-buzz interference; but in general experiments on combining two tasks have not distinguished between physical simultaneity of the stimuli and alternation of stimuli from the two tasks. There is therefore much room for further experiment on the view that an 'automatized' task is being carried out on the minimum amount of information, and that it interferes with other tasks only at the instant when that information is presented. But existing evidence favours such a view.

This work owes much to Sir Frederic Bartlett, F.R.S., Dr N. H. Mackworth and Dr E. C. Poulton, for general encouragement and advice, to Miss V. R. Cane on the statistical side, and to the Royal Navy for supplying subjects and equipment.

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MIND WANDERING

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The experiments here briefly described were primarily an attempt to devise and evaluate methods for investigating certain basic characteristics of attention. The two questions we asked may be put as follows: (i) How often does the mind wander? (ii) Does the frequency or duration of the mind wandering vary in any systematic way? The experiments were intended as preparatory to a search for the factors determining the presumed variations.

Three methods were employed:

(*a*) Each member of a class listening to a lecture, or of a group listening to gramophone music, was provided with a bell-push and instructed to press it whenever he became aware that his attention had returned to the lecturer or the music. The bell-pushes were so arranged that when any one was pressed, a corresponding torch-bulb lit up in an adjoining room and was recorded either on a continuously moving film or by observers. By this means up to fifty subjects could be studied at the same time.

(*b*) Observers watched a class of normal children through a one-way screen. They noted when a child seemed not to be attending to the teacher, the criteria of inattention being, for example, looking out of the window, talking to another child, fidgeting, falling asleep and so on.

(*c*) Observers watched a small class of deaf children through a one-way screen. Attention to the teacher required lip-reading on the part of the child, and the observers noted when each child was not looking at the teacher.

Methods (*b*) and (*c*) were indirect and enabled us to estimate how long each child's attention wandered on any occasion. Method (*a*) was direct, though it yielded only the frequency of wandering.

We found that in the lecture classes the subjects, on the average, pressed the bell-push about four times during a 40 min. period. The frequency was a little higher in the music group. In both cases individual differences were relatively enormous. Of the 165* subjects in the lecture classes, 14% did not press the bell-push at all, about 9% pressed it once, and four subjects pressed it twenty times or more. The mode was about three or four in the different lectures. Much the same results were obtained with the group listening to music.

Table 1. *Mean frequency with which bell-push was pressed during lectures, in 5 min. intervals from commencement* ($N=165$)

Minutes ...	5	10	15	20	25	30	35	40
Mean	0·27	0·52	0·56	0·61	0·62	0·64	0·55	0·43
S.D.	0·62	0·81	0·86	0·94	0·88	1·11	1·07	0·83

As may be seen from Table 1 the mean frequency with which the bell-push was pressed during the lectures, in intervals of 5 min., ranged from 0·27 to 0·64, the corresponding standard deviations ranging from 0·62 to 1·11. In the case of the music groups the mean frequency of pressing ranged from 0·18 to 0·87, with standard deviations ranging from 0·46 to 1·47 (see Table 2). Apart from the infrequency of pressing during the initial 5 min., no significant variation in frequency of pressing with time was observed.

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* Built up in four sessions with a single group of about forty.

Table 2. *Mean frequency with which bell-push was pressed by members of music group, in 5 min. intervals from commencement* ($N = 33$)

Minutes ...	5	10	15	20	25	30	35	40	45
Mean	0·18	0·67	0·76	0·82	0·58	0·82	0·87	0·65	0·80
S.D.	0·46	0·94	1·02	1·47	0·82	1·19	0·90	0·87	1·07

Subjects reports revealed the considerable difficulties many of them had in carrying out the instructions. Pressing the bell-push was found to be distracting and itself to divert the attention. It was often hard to decide when the attention seemed to have returned. Any particular wandering might feel like a definite lapse or daydream or, at the other extreme, like a slight oscillation or mere flicker of attention. When the mind wanders it may be to ponder more deeply on the lecturer's theme without being a lapse in any sense or it may be due to a 'pull' from some other direction which has nothing at all to do with the topic of the lecture.

In the experiments with twenty-nine normal children, judging by the criteria we employed, a child's attention seemed to return, on the average, about once a minute. The means for 5 min. intervals ranged from 4·95 to 5·73, with standard deviations ranging from 2·45 to 3·33. The duration of the wanderings on this basis ranged from a momentary flicker too short to be timed to about 3·75 min., with a mean of 13·74 sec. The details are set out in Table 3. Rather similar results were obtained from the observations of the deaf children by method (*c*), see Table 4. The mean duration of their inattention was about 4·8 sec.

Table 3. *Mean frequency with which the attention of normal children seemed to return, in 5 min. intervals* ($N = 29$)

Minutes ...	5	10	15	20	25	30
Mean	5·42	5·21	5·68	4·95	5·38	5·73
S.D.	2·92	2·67	3·33	3·47	2·45	3·13

Table 4. *Mean frequency with which the attention of deaf children seemed to return, in 5 min. intervals* ($N = 4$)

Minutes ...	0–5	5–10	10–15	15–20	20–25	25–30	30–35
Mean	4·75	6·25	5·25	7·25	4·0	5·0	4·5
σ	1·61	2·15	6·93	4·03	2·35	1·87	2·68

Further use of direct and indirect methods on the same subjects, with the experimental material and conditions held constant, may enable us to validate these methods. If the two types of methods yield similar results, then the simpler and more expeditious direct method of bell-pushing might usefully be employed in investigating the two questions formulated above and allied problems.

We are indebted to Prof. Ewing for providing facilities to observe the deaf children.

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THE INFLUENCE OF MENTAL SET ON PROBLEM-SOLVING

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In his laboratory manual, Foster (1923) includes an experiment which aims to demonstrate the influence of mental set on speed of problem-solving. He uses six different lists of five-letter anagrams. Two series, containing miscellaneous words, are presented with an indefinite set, the subject being told nothing about the general nature of the words involved. Two series comprise words which 'have something to do with eating' and another two series contain words 'representing things about the house'. Before attempting these, the subject is informed of the classification. Foster reports only that 'typical results show that the median time with indefinite set is about 2·5 times as long as for definite set' (1923, p. 152) and that 'a typical median time for transposing anagrams with indefinite set is 25 sec.; for transposing anagrams referring to eating, 10 sec.; for anagrams referring to house, 10 sec.' (Foster & Tinker, 1929, p. 6).

It seems likely that these results demonstrate the facilitating effect of definite as opposed to indefinite set. Nevertheless, an unambiguous interpretation is difficult to make. There is no adequate control of practice and fatigue effects. Also the specified and unspecified series employ different anagrams and there is no guarantee that the difference between the times taken to solve these is not attributable to a mere difference in their intrinsic difficulty. This note reports a class experiment in which Foster's materials and design were modified. The result demonstrates, of course, only one of the ways in which set can be established, and only one of the ways in which its effects can be observed (other investigations of set have been reviewed recently by Johnson, 1955). However, the experiment confirms, under better-controlled conditions, Foster's original findings. Moreover, it provides an interesting and instructive experiment for use in the teaching of experimental psychology.

The experiment was carried out with four laboratory sections of a first-year class in experimental psychology. A total of seventy-two students (thirty-five men, thirty-seven women) took part. The students were divided into groups of three, each member of the group acting, in turn, as subject, experimenter, and recorder. The material consisted of fifty-four five-letter anagrams, each of which permitted of only one solution. The solutions were nouns which fell into nine different categories as follows:

1. Animals: sheep, horse, tiger, skunk, camel, zebra.
2. Trees: birch, rowan, aspen, beech hazel, holly.
3. Musical instruments: piano, flute, cello, organ, banjo, viola.
4. Birds: stork, eagle, goose, robin, heron, raven.
5. Fruit: peach, grape, olive, lemon (melon), prune, apple.
6. Parts of the body: wrist, mouth, waist, ankle, thigh, chest.
7. Countries: India, Spain, Wales, Italy, Japan, Tibet.
8. Wearing apparel: glove, skirt, apron, scarf, dress, shirt.
9. Flowers: pansy, lilac, daisy, tulip, lupin, poppy.

Each subject attempted two lists of six anagrams, one specified and the other unspecified. The specified lists used, for example, by the first group were lists 1, 2, and 3 above, while each unspecified list contained six nouns, one selected from each of the remaining categories. Using twelve groups of three subjects, lists were prepared so that: over the thirty-six subjects, each noun occurred the same number of times, i.e. twice, as specified and as unspecified; practice and fatigue effects were counterbalanced, half of the subjects attempting a specified list followed by an unspecified, and half attempting first an unspecified and then a specified list; in no group did an experimenter or recorder experience a noun which he would encounter while acting as subject.

Each group was given a six-page booklet, on each page of which the appropriate list was typed in capital letters on heavy white paper. Each list was headed either 'Unspecified' or given a specific title, e.g., 'Animals'. Over the first list, the experimenter placed a long strip of hard-board in the centre of which was an aperture just large enough to expose a single anagram. The experimenter then moved the aperture down to expose the heading and told the subject whether the list was unspecified or specified in a particular way. He then exposed the first anagram for 30 sec. If the solution was given in this time, the experimenter waited until the recorder had noted the time taken for solution and then exposed the second anagram. If the anagram was not successfully transposed in 30 sec., a failure was recorded and the next anagram exposed. This procedure was continued until the end of the list. The same subject then attempted the second list. Members of the group then exchanged functions and the next two lists were presented to a new subject. This done, the third subject attempted the last two lists. Throughout, the recorder was responsible for timing the experiment by stop-watch and for entering the results in a specially prepared table. An hour gave the students time to perform the experiment, calculate the results for their own group of three, and discuss both their findings and the design of the experiment.

The results from the seventy-two subjects were as follows:

Median time to solve anagrams in specified lists = 3 sec.
Median time to solve anagrams in unspecified lists = 12·5 sec.
Total number anagrams failed in specified lists = 35 (8·10 %)
Total number anagrams failed in unspecified lists = 129 (29·86 %)

These results show clearly that the more specific the set the easier is the problem to solve. It is to be noted that the set involved here may not derive entirely from the instructions given but may, in part, have been established in the course of transposing a series of homogeneous anagrams. Even without specific instructions, a subject might, after solving four or five 'animal' anagrams, be set for an animal name in the sixth anagram. Such a task-induced set can easily be demonstrated in the classroom by means of the water-jar problems of Luchins (1946). However, the design of the present experiment does not permit any conclusion regarding the extent to which set established in this way contributes, if at all, to the results obtained.

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CRITICAL NOTICE

BY W. RONALD D. FAIRBAIRN

The Technique of Psycho-Analysis. By EDWARD GLOVER. London: Baillière, Tindall and Cox. 1955. Pp. x+404. 35*s*.

This volume, like Caesar's Gaul, is divided into three parts. Part One consists in an expanded version of the book published by the author in 1928 under the same title as the present volume and comprising six lectures delivered as a training course at the London Clinic of Psycho-Analysis in the days before systematic instruction in psycho-analytical technique had been initiated. The original book was published at a time when Freud's ego-psychology had only comparatively recently been formulated and his concepts of the super-ego and the id were still novel; and, in view of the considerable developments which psycho-analytical theory has undergone since that date, the author has had to consider how far the general outline of the original book could be preserved. In deciding to preserve it, he has been influenced by the consideration that, whatever complications may have been introduced into the technique of interpretation by theoretical developments, the 'analytical situation' as such and the phenomena of 'transference' continue to be governed by a few simple laws. He has also been influenced by the consideration that the practical difficulties experienced by students of psycho-analysis remain more or less constant. Part Two comprises an analysis of the results of a questionnaire concerning the details of psycho-analytical technique completed by twenty-four practising British psycho-analysts in 1938. The results of this research were originally published in 1940 under the title, *An Investigation of the Technique of Psycho-Analysis*; and they may be regarded as providing a representative picture of the practice of members of the British Psycho-Analytical Society at the date of the Questionnaire. Part Three contains three papers of varying dates, each of which has already appeared in *The International Journal of Psycho-Analysis*; and each represents a contribution towards an attempt to determine what is and what is not to be regarded as psycho-analytical treatment in the strict sense.

The book as it stands represents the most systematic and comprehensive study of the technique of psycho-analysis known to the reviewer; and, if the disquisition in Part One may appear to the reader somewhat *ex cathedra*, it must be remembered that this part originated as a lecture course for students of psycho-analysis seeking authoritative guidance in a field beset with difficulties. The reader can hardly fail to be impressed by the complexity of the problems which confront the psycho-analyst, and by the high standard of knowledge, training, judgement and skill required of him. However, Dr Glover takes pains to discourage any tendency to regard psycho-analysis as an esoteric cult, and to disillusion such as may be inclined to accept the myth of the psycho-analytical superman. As he makes clear, the psycho-analyst is merely an ordinary human being who, through his own analysis, has acquired an unusual degree of insight into the nature of unconscious mental processes and has achieved an appreciable mitigation of his own inner conflicts. In his view, any extravagant claims for the superiority of the analysed person were administered a *coup de grâce*, once and for all, by Freud's discovery of the id as an integral part of human nature; and he deprecates any inclination on the part of the analyst to identify himself with 'the all-powerful parent who knows everything and does no wrong'. He likewise makes it plain that, whilst the analyst must necessarily be dispassionate in his approach to his patient's problems and avoid the pitfalls of the counter-transference, he must never fail to maintain a human approach. Considerable space is naturally devoted to problems of interpretation, this being the main therapeutic instrument of the practising analyst; and emphasis is laid on the general principle that 'interpretation of unconscious phantasy must as a rule be timed to take effect when the defensive systems have been really weakened'.

The problem considered in Part Three, viz. that of differentiating psycho-analytical therapy proper from other forms of psychotherapy, obviously makes a special appeal to Dr Glover. Among the criteria of psycho-analysis to which he attaches special importance are (1) integrity and non-tendentiousness of interpretation, and (2) the maintenance of an attitude of detachment or neutrality on the part of the analyst. He also considers it as distinctive of psycho-analytical treatment that it aims not so much at strengthening ego-defences as at penetrating them with a view to unmasking unconscious processes which distort 'normal ego-defences', and that, in pursuing this aim, it abjures both suggestion and any unchecked exploitation of rapport. He further claims that any interpretative technique which is not based upon unreserved recognition of such fundamental Freudian concepts as repression and infantile sexuality culminating in the Oedipus situation forfeits all claim to be called psycho-analysis, and that the same applies to any form of psychotherapy which dispenses with analysis of the transference.

According to Dr Glover any therapeutic effects produced by 'inexact interpretation' must be regarded as dependent upon suggestion; and, indeed, he appears to regard suggestion as a form of therapy based upon an exploitation of inexact interpretation. It may well be asked, however, how far the effects even of 'exact' interpretation can be regarded as wholly independent of the authority and the general attitude of the analyst; and such a query is all the more justified in view of the fact, revealed by the results of the Questionnaire, that, whilst 'a number of orientated and practising analysts holding to the fundamental principles of psycho-analysis varied in their methods in every imaginable way', the therapeutic results obtained appeared to be 'much the same' (p. 373). This remarkable finding is perhaps less remarkable than a conclusion registered by Dr Glover regarding the perplexing problem of the length of psycho-analytical treatment, viz. 'that the earlier analysts were accustomed to conduct analysis of six to twelve months' duration which as far as I can find out did not differ greatly in ultimate result from the results claimed at the present day by analysts who spin out their analyses to four or five years' (p. 383). This conclusion may well give rise to some heart-searchings. The obvious doubt which it raises in the critical mind is whether what the earlier analysts did and what contemporary analysts do are really the same thing. This is a question which it is difficult to answer on the basis of available data, owing to what Dr Glover describes as 'the curtain of uncommunicativeness behind which psycho-analysts are only too prone to conceal their technical anxieties, inferiorities and guilts' (p. vii); and it should be noted that Freud himself provided comparatively little information regarding the technical details of his handling of patients. However, it is difficult to see how this question can be satisfactorily answered until more light is thrown upon the more basic question how psycho-analytical treatment produces its effects. The view current in the earlier days of psycho-analysis was that the therapeutic effects of analysis were produced by the dissolution of more or less localized 'complexes' combined with abreaction of the affects bound in these complexes; but this was in the days before Freud's ego-psychology had been formulated. The view most widely held in contemporary psycho-analytical circles and endorsed by Dr Glover himself is that the therapeutic effects of analysis depend upon (1) modifications of ego-defences such as to enable the ego to develop more satisfactory methods of dealing with id impulses (the id itself being, of course, by hypothesis incapable of modification), and (2) modifications of the super-ego such as to render it less primitive and less exacting in its demands upon the ego. However, among those influenced by Kleinian thought there is a tendency to attach therapeutic importance to the introjection of the analyst as a good object. The importance of this factor is regarded by Dr Glover as having been exaggerated. He is, of course, well known to be unsympathetic towards the Kleinian school; but he himself describes 'the humane relation of the transference' (p. 370) as a factor in the therapeutic process, and makes himself responsible for the suggestion that 'in the deeper pathological states a prerequisite of the efficiency of interpretation is the *attitude*, the true unconscious attitude of the analyst to his patients' (p. 372). All this suggests that the analyst does not merely fulfil the dual functions of a screen upon which the patient projects his phantasies and a colourless instrument of interpretative technique, but that his personality and his motives make a significant contribution to the therapeutic process. Dr Glover himself says in one place, 'It is obvious that many people cure themselves through their unconscious human contacts' (p. 372); and the question therefore arises whether the actual relationship between the patient and the analyst in the analytical situation is not the decisive therapeutic factor. If this be so, it may partly explain not only why analysts using varying methods obtain similar results, but also why the results obtained nowadays in analyses lasting four or five years are no better than those obtained in analyses of six to twelve months by the earlier analysts. For it may well be that the attitude of the contemporary analyst, steeped as he is in ego-psychology and impressed as he cannot fail to be by the inherent difficulties of effecting a lasting alteration in mental structure, makes a less therapeutic impact upon the patient than the more naïf attitude of the earlier analyst who regarded his task as comparatively simple and really believed that psycho-analysis provided an answer to all human ills. At any rate, it would appear an urgent demand that Dr Glover's masterly study of the technique of psycho-analysis should be supplemented by some more profound research than has hitherto appeared into the fundamental problem how psycho-analysis really works.

PUBLICATIONS RECENTLY RECEIVED

An Introduction to Psychology. By HARRY W. KARN and JOSEPH WEITZ. New York: John Wiley and Sons; London: Chapman and Hall. 1955. Pp. xi + 315. 32*s.*

Both for what it achieves, and for what it attempts to do, this book merits attention. Its aim is to interest, and impart accurate information to, the student undertaking only a *brief* course in psychology. Precisely because it is simpler and considerably less technical than the usual first-year text-book, this volume may meet the need for a book suitable as preliminary reading for the intending student of psychology.

This well-written book contains many illustrations, including line diagrams of apparatus, and makes frequent reference to specific experiments. The two introductory chapters place emphasis upon scientific method, and succeeding chapters deal with such subjects as perception; remembering and thinking; frustration and emotion; personality; and instruments of measurement. On occasion greater simplicity has been achieved by the authors' decision 'to take one systematic position and stick to it'; thus learning is treated from a reinforcement theory standpoint. Certain criticisms might be made. For example, the account of physique and temperament pays scant attention to the findings of actual investigations in this area. Again, the functional psychoses are discussed in an over-generalized and unconvincing way; moreover, it is difficult to see how a book which does not mention hallucination can deal adequately with a topic like 'schizophrenia'. Somewhat unusual is the book's avoidance of any reference to Freud and psycho-analysis, and, although perception is dealt with, there is no mention of *Gestalten* or the Gestalt school.

Although this book contains little that will be new information for the psychologist himself, he may well wish to recommend it, not only as 'summer reading' for intending students, but also to his friends and non-psychologist colleagues. It is an informative, interesting, and lucidly written introduction to the subject.

PETER MCKELLAR

Scientific Method in Psychology. By C. W. BROWN and E. E. GHISELLI. New York and London: McGraw-Hill Book Company, Inc. 1955. Pp. ix + 368. 45*s.*

This new book by Brown and Ghiselli is simple, straightforward, clear, and written at the level of the typical American undergraduate student. It sets out precisely and without any sesquipedalian elaboration modern notions of scientific method, interspersing the discussion with many relevant and well-chosen examples. At its level the book does an excellent job and may be thoroughly recommended. It would be invaluable, particularly for arts students with little knowledge of the scientific method who have taken up psychology for the first time and need a gentle but firm introduction to the scientific way of thinking.

On the debit side are certain features which follow inevitably from the plan and purpose of the book and from the level at which it is written. Views are stated rather than debated, even when alternative views are possible. Certain important concepts do not appear at all, although they play an important part in modern thinking. (We may instance 'operational definition' and 'intervening variable'.) Some of the chapters dealing with psychophysical methods and elementary statistics might, with advantage, have been sacrificed for a fuller discussion of some of these points. These chapters are inevitably too short to be sufficient without further reading, and would normally be treated in a separate course anyway.

On the whole, however, the good points of the book very much outweigh those criticized, and *Scientific Method in Psychology* fills an empty place in the teaching of psychology in a more than adequate manner.

H. J. EYSENCK

Clinical versus Statistical Prediction. By PAUL E. MEEHL. Minnesota University Press; London: Cumberlege. 1955. Pp. x + 149. 24*s.* net.

Much of the research designed to illustrate the relative success of clinical *vis-à-vis* statistical prediction has been vitiated by the lack of clarity with which these seemingly unambiguous terms have been employed. It is interesting that Dr Meehl succeeds in finding only twenty studies in which the two methods are sufficiently well differentiated for meaningful comparisons to be made.

The theoretical analysis of the problem is carried out with the author's customary care and rigour. After reminding the reader of the difference between the validating and the structural or analytic use of statistics, he suggests that a frequent source of confusion in past work has been in the failure to distinguish between the way in which data have been obtained, and the method by which they are combined to arrive at the required prediction. Psychometric data—the results of properly standardized tests—combined mechanically, i.e. by an application of an equation or table to the data, afford a prediction which has unquestionably been derived by a 'statistical' process. When non-psychometric data are combined non-mechanically or informally—as, for example, when an employer, after studying work histories and the foreman's report, conducts an interview to decide the most suitable candidate for promotion—such a procedure is clearly 'clinical'. Between these two clear-cut examples we have the less easily classifiable cases where psychometric data are combined non-mechanically, non-psychometric data are combined mechanically, and finally the example typified by most case-conferences where the data, psychometric and otherwise, are combined informally to yield a diagnosis and prognosis.

Dr Meehl continues his analysis by considering the view of Lundberg, Sarbin and others that the processes by which the clinician moves from his data to a prediction are logically indistinguishable from the mechanical application of an actuarial table or regression equation to the same data. What differences there are are quantitative rather than qualitative, and favour the overtly statistical method, since its operations are explicit and its units precise and well defined. If the clinician arrives at his conclusions by a process which is essentially if implicitly actuarial, he lays himself open to the charge that he is functioning as 'a second-rate substitute for a Hollerith machine'. One feels that Meehl finds this charge proven, but provides, for the philosophically sophisticated at least, a possible plea for leniency in the non-frequentist logic of Carnap. The practical implications of this point of view lead one to expect that when the two methods are compared the statistical should be found to be more reliable and more efficient. The evidence reviewed here adequately confirms this expected superiority of the actuarial method.

The scope of the studies cited is however rather limited, dealing largely with the prediction of scholastic success or failure, recidivism, and the recovery from psychotic illness. This is not a very adequate cross-section of the clinician's predictive activity, and of course the clinician as therapist is outside the scope of the discussion. Our doubts about the conclusiveness of the evidence examined are strengthened when we consider that in these studies the actuarial methods used are refined, reliable processes of known value, whereas we have no information about the quality of the clinicians with whom the comparisons are being made. These limitations, fully recognized by Dr Meehl, are inherent in the available research material, and should not be considered an inadequacy in the book. Indeed, despite the admittedly sketchy empirical evidence, one feels that the burden of self-justification has been transferred to the clinician. There remains, however, a real need for carefully planned research in this area; the subtlety and flexibility of the clinical approach is perhaps unduly constrained by the need to predict recidivism and psychotic recovery. A more congenial research project may provide the clinician with a better opportunity of demonstrating the effectiveness of his approach.

The clinician and the statistician, however fiercely they may debate this issue, do so with the assurance that whatever the outcome they will be only slightly affected by it professionally; this happy position is not shared by the clinical psychologist. His professional status and success are directly dependent on the effectiveness with which he can make predictions. Unfortunately the psychometric tools and actuarial tables which would permit him to adopt a purely statistical approach have not yet been designed; while his psychiatric colleagues rightly expect more of him than the clinical experience and insights on which they rely. He is thus obliged either to restrict the range of his activities—not always a possible solution —or to effect some compromise in his methods. In practice he may have to supplement a relatively narrow range of diagnostic instruments, with the more or less inspired guesswork which passes for clinical intuition. To the scientifically trained psychologist this is a most uncomfortable state of affairs. However significant his contribution to the case-conference, he will remain rather a man without a country, until the controversy which Dr Meehl so brilliantly presents has been satisfactorily resolved.

J. DREWERY

The Child's Conception of Reality. By JEAN PIAGET, translated by MARGARET COOK. London: Routledge and Kegan Paul. 1955. Pp. xiii + 389. 25*s*.

The psychology of childhood is beset with pitfalls, and overwhelmed with 'material'. The manual workers, industrious ants, have indeed amassed a colossal heap of facts—physiological, behavioural, psychometric, normative—and the clinicians a deal of artful rule of thumb, but little of it making any effective contribution to the development of a properly genetic psychology. This must, nevertheless, remain one of the foundation stones of a sound general psychology—essential, for example, to a proper appraisal of Michotte's work on causality, or of the Gestalt account of perception. That is why Piaget's latest adventures over the ill-charted sea of childhood are so important, and that not only, or indeed primarily, to the students of child development as such, but rather to the general psychologist.

The present book is a companion volume to the earlier *Origin of Intelligence in the Child*, and should be read in conjunction with it. In both the data are drawn from Piaget's carefully recorded observations—after the manner of Stern and Valentine—of his own three children during the first two years of life. These data are now analysed and re-analysed in order to demonstrate the pattern of development of (*a*) the object concept, (*b*) the spatial field, (*c*) causality and (*d*) the temporal field. The whole is summarized and brought together in a final chapter entitled 'The elaboration of the universe'. In the earlier work Piaget has analysed the origins of intelligence in order to elucidate how the forms of intellectual activity are constructed on the sensori-motor level. He now sets out to show how the real categories of sensori-motor intelligence are organized—'how the world is constructed by means of this instrument (of intelligence)'. Consequently the works stand as a vindication, as it were, of intelligence at the earliest stages, a firm assertion of 'the primacy of intellectual activity over ready-made structures, a primacy which Gestalt psychology seems...to neglect'. Piaget, therefore, is concerned first to enable us to understand how the child comes to the apprehension of external objects considered separately as items relatively detached, and then to demonstrate how by the same six stages (cf. *The Origin of Intelligence*) they come to be interconnected as parts of one world. The aim is to trace the movement from the vague and rudimentary apprehension of 'thing' or of space, tied to or implicit in behaviour that is initially reflex in character, to the acquired unity and differentiation of the external world and of the self as part of the world.

No psychological enterprise is more worth while; none is more difficult, and we may not complain if the discussion is difficult also. There is, indeed, an element of almost Chestertonian paradox in the simplicity of the observations—'games with children'—and the abstract level at which the argument is sustained. As Valentine has pointed out, the most reliable evidence as to what is genuinely innate in human nature, and what in the taken-for-granted bedrock of our experience is to be regarded as second nature, must be found in the study of human infancy. But this involves us in the closest scrutiny of the evidence that is produced, and warrants the most cautious acceptance of the interpretations that are based upon it. Piaget has never been afraid of bold and imaginative theorizing, and the new Piaget is no less courageous, no less imaginative, though less facile than the old. Yet there are certainly occasions where we may be permitted to doubt the inevitability of the interpretations he himself favours. For example, in Obs. 51 (p. 58), which turns upon a child's understanding (or misunderstanding) of adult language, the elucidation appears to be precarious. It is perhaps doubtful also whether the factual material presented is generally sufficient to bear the superstructure that has been erected upon it. Nevertheless, the evidence upon which we shall rely must be of the kind that Piaget has collected, and further testing and observation must follow the lines of his prolific suggestion.

No brief review can do justice to this important book. Along with the earlier volume, it constitutes the most important contribution to the genetic approach in psychology that we have had from any writer in Europe or America for a quarter of a century. It is perhaps a pity that the difficulty that most readers will certainly experience should be increased by some oddities of the translator's vocabulary and the appalling length of some of the trailing sentences.

GEORGE SETH

Theories of Perception and the Concept of Structure. By F. H. ALLPORT. London: Chapman and Hall. 1955. Pp. xxii + 709. 64*s*.

This book is written for psychologists who are interested in theory. As the title indicates, it has the dual purpose of surveying contemporary theories of perception and of launching a General Theory of Event Structure. This theory, introduced in the final chapter, is still in such a tentative and almost purely logical state of development that the author promises a more extended treatment of it in a forthcoming book. The theory starts from the fact that, in science, quantitative laws are not enough. It is also necessary to know something about the non-quantitative 'general format' or 'structure' of phenomena. This being so, it may be possible to derive 'structural' laws which go beyond mere qualitative description and are logically prior to quantitative relations. The author claims that such laws are conceivable and attempts to give some inkling of what they may be like. Because the theory is so undeveloped, it is difficult to evaluate. If the author's claims are justified, it will revolutionize the thinking not only of psychologists but of scientists in general and also, presumably, of historians. But, as presented in this book, the concept of structure is not too clearly delineated; the reviewer feels that, so far, the theory says nothing which has not been said before in simpler terms; he is not even convinced that the problem of 'structure' is, in any sense, a real one. A final assessment of Allport's theory must await the appearance of his second volume.

The major portion of the book is devoted to a review of thirteen theories of perception. These theories are: Titchener's core-context; Gestalt theory; Lewin's field-theory; Hebb's cell-assembly; Werner's sensori-tonic field; Freeman's set; Helson's adaptation-level; Brunswick's probabilistic functionalism; Ames's transactional functionalism; Bruner and Postman's directive-state; hypothesis-theory; Hull's S-R theory; and cybernetic theory. As a survey, this part of the book—long enough to be a book in itself—suffers by comparison with, say, Hilgard's *Theories of Learning*. It disappoints on three counts. First, many of the theories are not theories at all, in so far as they aspire merely to offer generalizations about some delimited aspect of perception. This would not be important but for the book's tendency to criticize each theory for its lack of comprehensiveness and so, in some cases, forgo a more detailed critique. Secondly, the treatment is insufficiently factual. Only with the discussion of the more recent 'new look' work (in chapter 15) is it concerned with relatively concrete perceptual matters—and this is the most stimulating and perhaps the most constructive part of the whole book. Thirdly, the survey is incomplete. For example, Michotte's name is nowhere mentioned, nor is that of Piaget, who employs many concepts of the sort favoured by the author. The crux of the matter seems to be that Allport does not intend to follow the Hilgard pattern. He surveys primarily to emphasize the need for 'structural' laws. Gestalten, cognitive maps, 'hypotheses', brain-fields—he seeks to show that these concepts, and many more, represent unsuccessful attempts to deal with the problem of 'structure' in the field of psychology. Yet, to the reviewer, this is not the moral pointed by the survey. To him, the moral, if any, is that psychologists should not expect to find a 'general theory' of perception with greater confidence than physiologists would, say, a 'general theory' of digestion.

Allport attempts to do something really important, and the time is ripe for a work such as this might have been. It is all the more regrettable, therefore, that the book's two aims should defeat each other. A clear statement of Event Structure Theory is crowded out by the review of other theories which, for their part, are inadequately treated because they must serve to orient the reader towards the final theory. Nor is the reader assisted by the style of presentation. Helpful diagrams (such as appeared in the much more satisfactory statement of the theory in the *Psychological Review*) are eschewed. The writing itself is diffuse; since its contents close the book to the legendary 'interested layman', the psychologist reader would have preferred a less repetitious and more cogent version.

Despite the above criticisms, the book contains many good things and strongly recommends itself to all psychologists who experiment on and theorize about the phenomena of perceiving, learning and thinking. In it are raised again those basic problems, both philosophical and scientific, which it is so often tempting to ignore. In it are provocative verdicts on viewpoints cherished by many. In it, too, the advanced student will find useful starting points at which to begin his study of modern contributions to the psychology of perception. And it cannot fail to give the perception specialist a broader perspective. Although the book may not fulfil the promise of its early pages, it represents, nonetheless, a valiant, sincere and skilful attack on some of the most perplexing problems in psychology and whets the appetite for its successor.

I. M. L. HUNTER

The Actual-Genetic Model of Perception-Personality. By ULF KRAGH. Lund: C. W. K. Gleerup; Kobenhavn: Munksgaard. 1955. Pp. 394. Kr. 40.

The term 'actual-genetic' is translated from the German 'Aktualgenese', invented by F. Sander to denote the successive perceptions of a stimulus object which is at first only imperfectly seen. In fact, of all the stages preceding full perception, only those are included in which there is some identification of form and attribution of meaning. It was further suggested by Sander that these stages bore some definite and consistent relationship to the final percept; and also to the structure of the personality. The author of this book has put forward the additional hypothesis that the 'actual-genetic' series of partial perceptions may reflect the actual stages of development of the personality in relation to the principal events of emotional life.

In investigating this thesis, the author has used two types of perceptual experiment: (1) measuring the changes which occur in the size of the after-image when the latter is repeatedly projected on to a distant screen; (2) recording the changes which occur in tachistoscopic perceptions of drawings, photographs of faces and T.A.T. pictures, with successive exposures of gradually increasing duration. It is difficult to understand what relationship the first experiment bears to the second group, and its results seem to give little information about personality beyond indicating that obsessionals show less tendency than do normals to a gradual modification in the size of the projected image. This seems to be paralleled, however, by the observations made on the tachistoscopic perception of drawings and photographs of faces, that obsessionals tend to give 'all-or-none' reactions rather than having gradually and continuously modified perceptions.

The results obtained with the T.A.T. pictures are more interesting. It does appear that the series of percepts followed somewhat the same line of development with different pictures; for instance, with one subject reports of a woman were consistently replaced at later stages by those of a young child, and later still of an older child. The author claims that these sequences were closely related to particular types of experience in childhood; thus orphaned youths tended to see a 'mother image' in the later stages of the series, where normal controls usually reported a 'father image'. Though case histories and free associations were obtained from many of the subjects, the correspondence with sequences of percepts has not been fully worked out.

It is arguable that these sequential responses may themselves come nearer to imaginative constructions than to true perceptions, though it is suggested that the subjects really thought that they saw what they reported. Whatever the relation to developments in personality, it is doubtful if they have any relevance to other types of perception. Certainly it cannot be concluded that all perception passes through an 'actual-genetic' series of stages which can be 'regarded as a micro-model of the ontogenesis of the personality'. It is possible that the author does not wish to claim as much; but his argument is obscure and hard to follow. Nor is it clarified by the arrangement of the book, which states the conclusions and discussion at the beginning, and describes the experimental methods and results at the end.

However, the method of examining sequential changes in perception appears to be a promising one; and these might well be studied on their merits and without regard to psycho-analytic interpretations.

M. D. VERNON

Experiment in Depth: a Study in the Work of Jung, Eliot and Toynbee. By P. W. MARTIN. London: Routledge and Kegan Paul. 1955. Pp. 288. 25*s*.

This is an excellent book, beautiful in the sheer economy with which many profound ideas and feelings are expressed in few words. It is mainly for the thoughtful layman, and for the psychologist who has specialized in fields other than Jungian analysis. Experienced Jungians will find much in it familiar, but there is a new and interesting attempt to explain the difference between men and women, which was never really explicable within the Jungian scheme.

Toynbee's thesis is that in periods of political trouble human beings turn inwards and find a solution in the depths of their own psyches. Martin sees the present world as divided into totalitarian states, which foster in the psyches of their subjects a split whereby people project all the bad on to their enemies and all the good on to themselves and their leaders, and free nations in which people have no sense of purpose, and so make no proper use of their freedom. He thinks it necessary for the citizens of the democracies to regain a sense of purpose by exploring their deep unconscious, a process which Jung has described scientifically and Eliot in poetry. This exploration leads to experience of a religious quality, and Martin thinks there is no ultimate conflict between science and religion, since the nature of such experiences can be verified when different people compare what they have undergone.

Experiment in Depth is well illustrated with dreams, with accounts of waking experiences of the growth of symbols, and with quotations from Eliot's poetry, and from many religious writers, including a number of Quakers. It will puzzle or anger the one-sided thinking type, which means that some psychologists will find it unscientific. It will stir other readers' feelings deeply, and arouse awe and a sense of the numinous by the mere reading of it. It throws unexpected light on familiar things: the present writer had never before realized that the grain of mustard seed and the pearl of great price were Self symbols.

Martin warns against many of the pitfalls in the exploration of the unconscious, and he is a good avoider of pitfalls himself. Although he is at times nostalgic for the Middle Ages, he knows that the Medieval Church was totalitarian. But he has some blind spots. He belongs to the feeling type, and, in a way characteristic of this type, he subtly undermines the religion he appears to be defending; for he writes, in effect, that it does not matter what God is like as long as people have religious experience on the feeling level. He knows nothing of the positive value of Freudian analysis, and nothing about small children.

The repeated cry for a creative 'minority' to save us seems odd to anyone familiar with the fresh and individual way in which every child approaches life, and it is odd to discuss the difference between men and women without once mentioning the difference in our bodies.

Martin recommends that people should explore their own unconscious without the aid of an analyst, by methods which range from meditating on their own dreams to spending a whole night alone out of doors. He thinks the dangers of the procedure must be accepted because of the desperate state the world is in. He does not mention the shortage of analysts, but it has obviously influenced his thought. It is not certain that his diagnosis of the main weakness of the democracies is right. Both militarists and pacifists appeased Hitler, and neither lacked a sense of purpose. However, the exploration of the deep unconscious can remedy other states of mind as well as purposelessness. The real value of *Experiment in Depth* will be known only after the passage of a number of years.

JANE DARROCH

FERENCZI, S. *Final Contributions to the Problems and Methods of Psycho-Analysis.* Edited by MICHAEL BALINT. The International Psycho-Analytical Library, edited by Ernest Jones, No. 48. London: The Hogarth Press and the Institute of Psycho-Analysis. 1955. Pp. 447. 30*s*. net.

This is the third volume of papers and contributions to psycho-analysis by Ferenczi to be published in the International Psycho-Analytical Library. It contains thirteen papers written between 1926 and 1933, after the publication of the previous volume, seven posthumously published papers, written between 1909 and 1922, notes and fragments written between 1920 and 1932 or undated, and papers, 'varia' and reviews omitted from previous collections and dated between 1908 and 1922.

The main contributions in this volume are important, and it is excellent to have them published in this way. All the material in the volume has been admirably edited and translated, and reads as though English had been Ferenczi's native language. It is very satisfactory to have the date of almost every item and to be able to follow the development of Ferenczi's thoughts in relation to the history of the psycho-analytic movement. The volume contains a bibliography and a 50-page index.

While the interest and importance of most of the papers cannot be doubted, it seems an open question whether even Ferenczi himself would have approved of the inclusion of all the notes and fragments, some of which seem as though they can hardly be intelligible even to a trained psycho-analyst. However, there is some psychological interest in a cross-section of the random thoughts of a great man.

R. W. PICKFORD

Psychoanalysis: Practical and Research Aspects. By WILLI HOFFER. London: Baillière, Tindall and Cox. 1955. Pp. x+102. 27*s*. 6*d*.

In 1953 Dr Hoffer was invited to give the Abraham Flexner Lectures at Vanderbilt University. Judging by this volume, containing the substance of his lectures, he has certainly maintained the high standards of the previous holders of this lectureship. Though small in size, it ranks with Freud's *Lay Analysis* as one of the best introductions to psycho-analytic theory. Indeed, Dr Hoffer has laid emphasis on the developments in psycho-analysis since Freud. In this country, any advance in psycho-analytic theory and technique has usually been identified with the work of Melanie Klein and her followers. Yet the contributions of Mrs Klein and Fairbairn pale beside the major contributions to scientific psycho-analysis made by Hartmann, Kris, Loewenstein, Rapaport and others in the United States, and Anna Freud, Mrs Burlingham and Hoffer in England. It seems very likely that when psycho-analysis, experimental psychology and medicine reach a synthesis, their meeting-place will be ego-psychology.

Hoffer introduces his topic by reference to the principles of psycho-analytic training, the concepts of internal conflict and anxiety, and recent contributions to the psycho-analytic investigation of childhood. He continues with a full report of a case of his own illustrating the ego-reactions in cerebral disease, and concludes with a chapter on some problems of ego-psychology. Dr Hoffer does not indulge in flights of fancy, but studiously attempts to define the concepts of modern psycho-analytic theory, and their relation to actual observation. This little volume, which is the most modern exposition of Freudian psycho-analysis, will prove an eye-opener to those who feel that psycho-analysis has parted company with medicine and science. It should prove of particular interest to those who are concerned with problems of the development of memory, thinking and perception. In addition it is admirably suited to the University student who wonders what psycho-analysis is all about.

Any criticism of this book cannot be of the psycho-analysis involved. Hoffer is one of the most eminent of the psycho-analysts trained by Freud, and one can only complain that the book is expensive for its size and that lectures given verbally may not read as easily as they are heard. JOSEPH SANDLER

The Object Relations Technique. By HERBERT PHILLIPSON. London: Tavistock Publications Ltd. 1955. Pp. x+224. 21*s.* net; with Test material, 3 guineas.

At first sight Mr Phillipson's contribution to the published range of projective techniques would appear to be little more than yet another variant of T.A.T. It has, however—in contrast to both T.A.T. and Rorschach, which are somewhat loosely based on an eclectic depth psychology—the distinguishing feature that it has been designed and developed as an adjunct to therapy carried out within the framework of a single theory of the development of personality—the object-relations theory of which Klein and Fairbairn are the best-known proponents. That being so, it is appropriate that the central chapter (extending to more than half) of the book should consist of illustrative case material. Of this chapter half, again, is devoted to detailed study of a single case, with very full interpretations of all aspects of the subject's response.

The test material consists of twelve pictures and (as in T.A.T.) a blank card. There are three series of pictures, distinguished by the style of drawing: *A*, in light charcoal shading, misty in quality; *B*, in heavy shading approaching a silhouette technique; *C*, with colour introduced, and more definite detail. In each series one picture presents a one-person situation, one a two-person, another a three-person and the fourth a group situation. The *A* series, it is claimed, 'will stimulate primitive dependent needs and the consequent anxieties'; in the *B* series 'the emphasis (is) on phantasy relation with threatening and uncompromising objects'; the *C* series is intended 'to present a world rich in opportunity and challenge'. How far this attempt to systematize stimulus material has been successful is not clear from the normative data provided. Thus, figures derived from two samples—of fifty clinic out-patients and of forty normal adolescent girls—show that direct references to colour are made in only about one-third of responses to any one picture; evidence on the relative specific functions of the *A* and *B* series, as defined above, is also lacking. The present reviewer has also formed the impression that the extreme ambiguity of, particularly, the *A* pictures may produce a Rorschach-type attitude (puzzling over detail etc.) which tends to inhibit both the production of a 'story' and the identification phenomenon stressed by Henry Murray. In view of the underlying theory this may not be important in the clinical situation proper, but if, as the flap description suggests, the material may also be used 'outside the clinical field, in social research and in personnel selection in industry and elsewhere' it may be that avoidance of structuration has been carried to excess.

One will await with interest further reports of the use of the technique (as promised, e.g. in articles listed as 'In preparation'). Perhaps it will not be regarded as too uncharitable also to hope eventually for a clearer account of its 'quantitative possibilities' to which Dr J. D. Sutherland refers in his Foreword. The author must, however, be congratulated on the degree of success he has achieved in his main purpose—the linking of the projective hypothesis with current thought in perception theory, and the application of both to a coherent system of personality dynamics. BORIS SEMEONOFF

Recent Developments in Psychosomatic Medicine. Edited by ERIC D. WITTKOWER and R. A. CLEGHORN. London: Sir Isaac Pitman and Sons Ltd. 1954. Pp. xvi+495. 50*s.* net.

As the Editors say of this book 'it is not meant to replace the admirable book *Psychosomatic Medicine* by Weiss and English. It is meant to serve the purpose stated in its title—to describe modern developments in psychosomatic medicine.' It is not as encyclopaedic as the new fourth edition of Dunbar, but it is more easily read. Twenty-nine authors contribute twenty-five chapters. All except three authors have medical qualifications and most have psychiatric appointments. Several are psycho-analysts.

Coverage is not limited to areas in which psychological factors have, in the past, been presumed of etiological significance. Medicine, 'in order to be good medicine must be psychosomatic irrespective of the extent to which psychological factors are of etiological significance'. Hence chapters on pulmonary tuberculosis, venereal disease and disablement have been included.

The book is in two parts—general and specific. The eight chapters in the former are likely to be of value to all psychologists whose work brings them into the medical situation, especially Chapter I, 'Basic

Concepts in Psychosomatic Medicine'; Chapter II, 'The Problems of Specificity in the Psychosomatic Process'; and the two chapters on research by S. G. Wolf and by R. B. Malmo.

In Chapter I, Macleod, Wittkower and Margolin present for comparison the underlying conceptual models on which the current theories of psychosomatic medicine are based.

Kubie's Chapter II is concerned with one of the basic questions to be faced by psychologist and doctor in this area—the problem of specificity. He ends with a question, 'What is there which is of a peculiar, or special or different or specific nature about the regression and dissociative processes which result in physiological disturbances?' Kubie refers to two long-term projects which will help to settle some of these questions; one in which investigators recorded physiological variations in children, who will be followed through coming years for their subsequent somatic and psychological histories, and another at Johns Hopkins Medical School where all medical students are now subjected to intensive psychological scrutiny, and through the coming years these doctors-to-be will send back to the study-group physiological data on their own blood-pressures, pulse rates, etc.

Malmo's Chapter V on 'Research: Experimental and Theoretical Aspects', argues for the importance of theory construction in this area and for the desirability of clinician and experimentalist working together. He outlines the value of electromyographic techniques when combined with clinical research.

MacLean's Chapter VI, 'Studies on Limbic Systems ("Visceral Brain") and their Bearing on Psychosomatic Problems', brings together material from comparative neurology and recent work on the frontotemporal region in relation to the subjective aspects of certain syndromes: '...one can make the general inference that the frontotemporal region is concerned with the oral activities of the animal as they pertain to feeding and to the vocalisation, attack, and defense involved in obtaining food. In an effort to go further and to obtain insights into the subjective functions of this part of the brain one derives a wealth of material from a clinical study of psychomotor epilepsy.' Kubie, in Chapter VII, groups the body involvements in psychological conflicts under four categories and draws on the work of Penfield and others. 'Consequently we are justified in saying that the temporal lobe, with its deeper primitive connexions, is a mechanism for the co-ordination of all the data which link us to the world of experience, both external and internal....It is the area of the brain which deserves to be viewed as the organ which is essential for psychosomatic relations.' E. L. Margetts contributes a valuable chapter, 'Historical Notes on Psychosomatic Medicine', in which he sums up the literature on the body-mind problem in medicine from ancient times to the present day.

Part II, 'Specific', will be of interest to the psychiatrist, but in both it and the preceding part he will need, if not some sympathy with dynamic psychiatry, at least a fairly advanced knowledge of its terminology. The psychologist will find most of these chapters useful as a beginning when asked to co-operate with the psychiatrist on a psychosomatic problem. There are chapters on diabetes mellitus; rheumatoid arthritis; skin diseases; cardiovascular disorders, heart disease and hypertension; peptic ulcer; non-specific ulcerative colitis; and psychosomatic disease due to battle stress.

The reviewer found the following particularly valuable: (*a*) Cleghorn on 'The Interplay between Endocrine and Psychological Dysfunction', (*b*) D. O'Niell on 'Psychological Aspects of Gynecology and Obstetrics', with its welcome caution which might have come earlier in the book—'Because analysis of mental content has illuminated much that was hidden in the causation of disease, it is tempting to suppose that this way lies the answer to the question [Why a person gets ill in a particular way]. But the determinants of the 'focus' of illness can well be of a different order and there is room for research on this theme. The prime need, in gynecology and obstetrics, as elsewhere in medicine, is for the hard currency of fact'; and (*c*) the chapter by the two psychologists on disablement.

This volume adds a little to the impression that psychosomatic medicine, while preserving the rather unfortunate adjective is 'becoming incorporated into the general body of medical knowledge', passing from the stage where it was largely propaganda, and statements about coincidental phenomena, to a period of combined research by clinician and experimentalist into the relevance of psychological phenomena in the etiology of disease.

JOHN C. KENNA

The Mental Hospital. A Study of Institutional Participation in the Treatment of Psychiatric Illness. By ALFRED H. STANTON and MORRIS S. SCHWARTZ. London: Tavistock Publications Ltd. 1954. Pp. xx+492. 35*s*.

This is a highly interesting account by a psychiatrist and a sociologist of their examination of the community structure of a rather unusual hospital. Formal and informal activities among patients, between patients and staff and among staff are reported and discussed. Much of the book is given over to detailed reports and accounts of what goes on in administrative psychiatry rather than in the psychotherapeutic attack on psychoses. This focus is epitomized in the catch-phrase 'the other 23 hours', in which treatment can be effectively sought, over and above the formal sessions between patient and doctor.

The centre of the investigation is a disturbed ward of fifteen chronic patients, mainly suffering from schizophrenia, but the interaction of the whole hospital community is highlighted. The enormous difficulties of personal and group attitudes of communication and relationship and of morale, aroused in trying to develop the positive therapeutic atmosphere in social interaction, are faithfully reported. The reader who has experienced the problems of the effects of institutionalization superimposed upon the effects of psychoses will readily understand the setting of the investigation and the significance of the difficulties, misunderstandings and disagreements which are revealed. It may well be that the more general reader will be merely puzzled that such things have not been obvious before, but he can find as much of interest in the problems of method as the clinical psychologist can find in the results.

R. M. MOWBRAY

Textbook of Occupational Therapy. By E. N. M. O'SULLIVAN. London: H. K. Lewis. 1955. Pp. x+319. 21*s.*

There is no doubt that a comprehensive text-book on occupational therapy is badly needed, and Dr O'Sullivan's book will go some way to meeting this need. Apart from the chapter on the history of occupational therapy, which is of general interest, it is difficult to decide precisely for whom the book has been written. It is too elementary for the trained worker and too detailed for the administrator or the general reader. If it is intended for students, the absence of any illustrations whatever is a grave handicap, and some of the reproductions of report forms are very poor.

The author makes a very proper plea for adequate occupational therapy accommodation in the hospital, but his sketch plans of an ideal occupational therapy department, where each craft is housed in a separate room, would seem to vitiate one of the great advantages of this form of therapy, namely, the possibilities it affords for the formation of groups. Patients in such a building as Dr O'Sullivan describes would have to be arbitrarily grouped according to craft and not therapeutic needs.

There is a large bibliography in which books written twenty years ago or more predominate. In the psychology section, for example, nothing later than McDougall and the 1935 edition of Boring, Langfeld and Weld is quoted.

RALPH HETHERINGTON

Maladjusted Children. By CHARLES L. C. BURNS. London: Hollis and Carter. Pp. x+81. 6*s.* net.

Dr Burns, the Senior Psychiatrist to the Birmingham Child Guidance Service, has written a simple and interesting account of the main problems, aspects and methods of dealing with children who are maladjusted. The book includes nine short chapters covering the broad types of maladjustment, clinical methods of child guidance, aspects of natural development, problems of sex in childhood, delinquency, adolescence, discipline and the relationship of education to mental health. It is clearly written, without undue technicalities, and is eclectic in its approach to psychotherapy, with a generally Catholic religious outlook. For parents and teachers it would be admirable.

R. W. PICKFORD

Psychopathology and Education of the Brain-Injured Child. Volume II. By ALFRED A. STRAUSS and NEWELL C. KEPHART. New York and London: Grune and Stratton Inc. x+266 pages. 6 dollars.

The original book of this title, now to be regarded as the first volume of a projected series, was published in 1947. It is well known for the authors' views on the diagnosis and treatment of some types of mental defect or abnormality encountered in children, which derive, as regards theory, chiefly from the Gestalt school. The present book is not merely a revised edition, but embodies subsequent thought and clinical experience without going over the former ground again. It contains much interesting and valuable discussion in its four central chapters, entitled 'Psychopathology of Perception', of 'Language' (treatment of aphasia is summary and reserved for a further book), of 'Concept Formation', and of 'Behaviour'. No new theoretical views are propounded, but an extension into the field of the brain-injured child with 'normalcy in I.Q.', but behavioural deviations, is made. The first chapter deals with the anatomy and physiology of the central nervous system, but would be irritating to a serious student of these subjects. The text attempts to cover too much detail for its eighteen pages, most of which are, in any case, occupied by drawings which are also overloaded with detailed legends. One cannot but regret the supersession of the simple, but perfectly adequate, chapter on these topics in the original book. In general, it must be said, the illustrations serve the book poorly. They are crude in conception, too expansive and numerous for their quality, and, on occasion, e.g. Fig. 26, p. 130, make nonsense of the text. They are, however, beautifully printed on excellent paper.

Among the final chapters and appendices should be noticed the contribution by Samuel Goldenberg on 'Testing the Brain-Injured Child with Normal I.Q.', which considers controlled experimental evidence, rather than the fruits of clinical experience and cogitation, and gives a clear exposition of the scientific weaknesses of the main diagnostic distinctions on present evidence. This is, of course, the lack of a satisfactory criterion of 'brain damage', which is independent of the psychological tests supposed to show it, and against which they can be validated. The standard neurological indicators are expressly rejected in the dubious cases, on quite reasonable grounds; but it leaves an awkward gap in the logical sequence, which is liable to a 'closure' effect and to achieve a nice circular symmetry. Until the association of psychological and neuropathological findings has been demonstrated in the same way as for the neurological ones, the value of the test findings for this purpose remains dubious. The 'exogenous-endogenous' distinction may still, however, be perfectly useful empirically for educational purposes, although some more evidence on this point, too, would be welcome. Dr Strauss is well aware of the theoretical shortcomings of his thesis, and makes a good case for his procedure; but it is a little disingenuous to say, 'This would appear to be the same type of reasoning that is used in the mathematical problem of factor analysis'.

J. A. L. NAUGHTON

Studies in Education: 7. The Bearings of Recent Advances in Psychology on Educational Problems. University of London Institute of Education: Evans Bros. 1955. Pp. viii +215. 7*s.*

Following on the last volume in this series, the London Institute of Education has again printed a number of lectures by different authorities centred round a single theme. Appropriately enough for the seventh volume of this series, there are seven lectures. We start with a paper by Prof. Rex Knight on 'Children's Needs and Interests'—contemporary psychological theories of motivation. Then come papers by Dr C. M. Fleming on the 'Child within the Group'—the bearings of field theory and sociometry on children's classroom behaviour; Mr Ben Morris on 'Mental Health in the Classroom'—the teacher's personality and problems of children's adjustment; Mr H. A. T. Child on the 'Backward Child'—the incidence, causes and treatment of backwardness; Dr W. D. Wall on 'Teaching Methods'—psychological studies of the curriculum and of classroom teaching; Prof. E. W. Russell on 'How Children Learn'—contemporary theories of learning; and finally Prof. P. E. Vernon on the 'Assessment of Children'—recent trends in mental measurement and statistical analysis.

Of the value of this book there can be little doubt. For the teacher and educationist, too busy with the practice of their craft to hope to keep in touch with the growing edge of psychological theory and research, it covers a reasonably high proportion of this most adequately. The technique of the spoken lecture and the written paper vary somewhat, and examples are not unknown of successful lectures which in print have been less than satisfying. This difficulty is not obtrusive here; in general, the book reads well and only occasionally is one reminded of the lectern. Indeed, in one respect (that of length) the reader is forced to wonder how *all* the contributions could have been single lectures. The four University contributors are, no doubt, showing their occupational skill when they produce chapters which are almost identical in length. If one assumes that they each spoke for forty minutes, then it appears that Mr Child and Dr Wall took just over an hour, and that Mr Morris was only just coming to his peroration at the end of two hours. No doubt Mr Morris's ample footnotes (and footnotes to footnotes) account for part of this, but hardly for all.

It is likely that the chapters on backwardness and on teaching method will have the greatest initial impact on the practising teacher, since they are most obviously orientated towards immediate practice as well as being supplied with ample references to research work on practical problems. But Russell's attempt to lead the reader through the labyrinths of contemporary learning theory, supplemented by Knight's analysis of motivation (and Fleming's elaboration of field theory), are likely to be welcomed by many to whom the diversity of theory has been a challenge too formidable to accept. The job of structuring the field for the benefit of those who must eventually *apply* theory is not attempted often enough. We must be grateful for this, not only to Knight and Russell, but also to Vernon. The final chapter, although ostensibly covering mental measurement in general, deals mainly with the concept of intelligence. It gives, in Prof. Vernon's inimitable way, the first account accessible to the general reader of his view of intelligence and its bearing on educational selection. The postulation of 'intelligences' A, B and C, derived to a considerable extent from Hebb and Piaget, is beautifully explained, and the implications are illustrated by reference to beliefs commonly held by teachers. These are taken from a questionnaire answered by a group of London teachers, the results of which show very clearly the confusion which exists in this field. It does not seem unlikely that some teachers hold a set of opinions which are mutually incompatible. This is not surprising after the confused controversies in the educational press during the post-war years. This chapter is a bold attempt to produce order out of chaos by offering a *via media* based on analysis of the known facts. No doubt it will be attacked from both flanks, but to the teacher it may well seem the most significant step forward in this area of educational psychology since Spearman's theory of *g*.

It is, perhaps, unfair to criticize a book of this kind for what it does not contain. But in a publication emanating from an Area Training Organization it is a pity that teacher-training does not receive the emphasis it needs. It is mentioned incidentally—for example, in Morris's long chapter—but a fuller and more eclectic treatment would have enriched the book. But in spite of this, we must be grateful to the London Institute of Education for a volume whose usefulness for many teachers will far outlast its paper covers.

STEPHEN WISEMAN

Psychology in Education. By J. B. STROUD. London: Longmans Green and Co. 1955. Pp. vi + 664. 32*s.* 6*d.* net.

This is a straight reprint of a book first published in the United States in January 1946. Internal evidence suggests that most of the book was written in the late 1930's, with certain later additions. The bulk of the references given are to researches prior to 1940, and no reference is made to work later than 1944. The book is now published in the United Kingdom in 1955.

This means that all post-war educational psychology is omitted, and some of these omissions are important. The opening chapter, which is quite an extensive review of the sociological aspects of education, is perforce much more general and discursive than a corresponding chapter written today would be, as it lacks the body of information accumulated in the past ten years in that field. Similarly, the section on number contains no reference to Piaget.

This is not to say, however, that the book is wholly useless. It is, indeed, a rather odd mixture; nearly everything has been put into it. An objective, scientifically based chapter on 'Individual Differences' is preceded by a rather old-fashioned one on the 'Foundations of Learning', though in the latter the use of the historical approach is one which could be more frequently used in such books. Scientific facts rub cheeks with educational moralizings. There is, in short, a mixture of good and bad, and though the volume is not one which can be wholly recommended there are several chapters in it which are soundly based and could be studied with profit.

JAMES MAXWELL

Statistics in Education. By MERLE W. TATE. New York and London: Macmillan. 1955. Pp. x + 597. 42*s.*

The intention of this book, as described by Prof. Tate in the preface, is to serve education students taking a one- or two-semester course. The approach to the subject is mainly 'through common sense and arithmetic'. The author is obviously aware that many students of education are not mathematicians. Thus many of the algebraic formulae are merely stated, though some are derived in the text and in an appendix, after the manner of Guilford.

Yet this is more than a mere cookery book. Its author respects the intellectual quality of the reader. From her introduction, which she opens with an epistemological discussion, to her final chapter on 'Statistical Inference' (in the reviewer's opinion the best in the book), Prof. Tate demands from her readers some interest in underlying philosophical problems and appreciation of logic. It is not that she goes deeply into these issues; rather she whets the appetite, and in the references following each chapter indicates where the solid fare is to be found.

In one respect the reviewer was disappointed. The treatment of analysis of variance, while covering rather simple examples of one- and two-way classification, the test for linearity or regression, and with some mention of multiple correlation, does not introduce the student adequately to the wider possibilities of experimental design.

In its coverage of statistical techniques the book is conventional, with numerous illustrative examples and diagrams. Factorial methods of analysis are omitted. With the exception of analysis of variance the treatment is rarely superficial. The student is made to realize that means other than the arithmetic exist and have their uses; that there is some point in squaring deviations from the arithmetic mean rather than some other origin; that the formula for the correlation coefficient makes sense; that any attempt, on statistical grounds or otherwise, to foretell whether a particular child will succeed or fail, is nugatory; and that confidence limits require careful interpretation.

An interesting feature is the description of experimental studies of the distribution of such statistical artefacts as t, F and X^2, after the manner of Snedecor (whose *Statistical Methods* this book in some ways resembles). For the non-mathematical reader, such studies should do much to imbue with meaning these statistical commonplaces, so often accepted and employed uncritically.

A. E. G. PILLINER

Crime and the Services. By JOHN SPENCER. London: Routledge and Kegan Paul. 1954. Pp. xii+306. 28*s*.

This is an account of a research into the effects of Service life on criminal behaviour. Of the many aspects of this problem, the investigator sets out to concentrate on two: the delinquent in the Services and the 'Ex-Serviceman' in prisons and Borstal.

The research was conceived not only as a testing out of criminological theories in a particular field setting but as a practical contribution to the prevention of delinquency, both military and civil, in the Services. This latter involved an investigation of the methods of correction employed in the treatment of Service offenders. Questions of screening, selection, the discharge of criminals from the Services and the criminogenic influence of the resettlement period for all the Ex-Servicemen are discussed as specially relevant.

Part I of the book presents an analysis of the problems of adjustment to Service life in the last war, an account of Service crime and criminals and the techniques used for their reformation. Part II begins by examining the problem of the Ex-Serviceman's readjustment in civil life and includes a chapter on the special difficulties of the ex-P.O.W. Up to this point Dr Spencer's method is the usual one of collecting and collating information and statistics, together with a critical survey of the existing literature. This is done comprehensively and assiduously even to the point of being over-written with an excess of references and footnotes of every conceivable relevance.

The remainder of Part II consists of a field investigation of three groups of Ex-Service criminals (100 prisoners at Dartmoor, a similar number at Maidstone and 82 lads undergoing training at Sherwood and Gringley Borstals). There is a further study of the after-care records of 200 discharges from various prisons. By comparison with the previous section this part of the book is much less impressive, and one can easily find fault with the sampling, the general presentation of the material, the thinness of some of the case histories, the naïve personality typing and so on. Dr Spencer is acutely aware of much of this and attempts to justify it by reference to such difficulties as the poverty of prison records. One would regard such arguments as double-edged.

Part III begins with a critical assessment of the effect of institutional life in the penal establishments referred to above in terms of the inadequacy of ordinary reformative methods for meeting the needs of the 'chronic delinquent'. Here especially one detects an expression of opinion arising out of Dr Spencer's field experience but going far beyond his presented data. His final conclusions, however, are cautious and balanced. He rejects equally firmly the hypothesis that Service life has itself a criminogenic influence on its participants and the claim that it is a cure for delinquency. He would, however, admit that Military Service, and particularly its wider social repercussions, may have unfavourable effects 'for certain types of personality in certain situations'. He emphasizes especially the effects of separation from family life and the significance of the demobilization period.

Finally, there is a useful Appendix on Methodology which should be read by all future research workers in the penal field and, for their sins, by those who are concerned with the compiling of official records.

Dr Spencer claims that his research had to be carried out 'piecemeal rather than in a logically coherent form'. To this should be added his admission that 'the by products of research appeared to the writer to be of equal interest to [its] main object'. This is only too clear from the loose organization of the book as a whole. One would suspect that Dr Spencer was faced with the problem of how to achieve a concise presentation of his material and yet satisfy his need to include every detail of his field experience and its impact upon him. Perhaps a better solution would have been for him to have written two books, one a more rigorously integrated presentation of his research findings and the other a more frankly personal account of the effect of his investigations on his attitudes and opinions.

Those, however, who can cope with the mild irritation which such defects may arouse and who will take the trouble to follow up Dr Spencer's many suggestive leads will finish up with a much broader understanding of delinquency in general and its treatment possibilities. From this latter point of view especially this book is certainly to be recommended.

R. L. MORRISON

A Survey Report on Human Factors in Undersea Warfare (1949) and *Supplement* (1954). Prepared by the PANEL ON PSYCHOLOGY AND PHYSIOLOGY, Washington, D.C. Committee on Undersea Warfare, National Research Council. Pp. x+541. Supplement, pp. i+92. No price given.

Approximately three-quarters of this book is devoted to the methods, findings, and some of the problems of what has now come to be called engineering psychology. Although special reference is made to conditions in submarines, the majority of the material is quite general in its applications, and about half of it is most useful.

The topics discussed include the psychophysics of vision, the reading of printed materials, the design of instruments for displaying information, fundamental considerations in designing controls, the design of working panels containing displays and controls, systems research, the effect of noise and distortion upon voice communication, lighting, and the shortcomings of current placement and training procedures. Among more physiological topics are temperature regulation, the sleep-wakefulness cycle, and motion sickness. A section is also included on stress, and the conditions relating to group effectiveness. The chapters on the night lookout, the sonar operator, diet, and possible methods of visual communication, may be of less interest to the general reader.

It is now nearly seven years since the book was first published, although the supplement has only recently become freely available. No mention is therefore made of more recent experimental work, such as that on visual watch-keeping and multi-channel listening, to name two examples. However, throughout the book there is a strong emphasis on the shortcomings of previous research, which was mainly conducted under the stress of wartime conditions. This emphasis is refreshing, and is still exceedingly relevant. In addition, the directions in which research could usefully proceed are described in some detail, and have not changed much in the last seven years.

E. C. POULTON

The Non-Human Primates and Human Evolution. Arranged by JAMES A. GAVAN. Detroit: Wayne University Press. 1955. Pp. 134. $3.50.

The papers in this symposium were presented at a 1953 meeting of the American Association for the Advancement of Science, and they come from anthropologists, psychologists, palaeontologists and comparative anatomists. The book is dedicated to the anthropologist Hooton who took part in the symposium, but died soon afterwards. There is no doubt that his contribution is by far the most provocative, and suggestive of future research. He makes a strong case for genetic studies on lower primates, and for more studies of infra-human primate societies which, he considers, are far more fruitful than 'the stuff on present-day savages written by anthropologists'. The book as a whole is aimed at explanation rather than description. The lucid chapter by the comparative anatomist, D. Dwight Davis, is perhaps the most successful in this respect.

There are three chapters by psychologists: Carpenter provides a useful summary of infra-human primate social grouping, better described as infra-human sociology; Nissen's chapter is concerned with explaining to non-psychologists the work of the comparative psychologists; and the chapter by Mr and Mrs Hayes describes the 'cultural capacity' of a chimpanzee, Viki, reared for six years in their home. This last is interesting in that several of the author's remarks, for instance on thumb opposition, posture and the use of tools, and brain size, are unorthodox.

It is stated that the papers have already appeared in *Human Biology*, September 1954.

BRIAN FOSS

The Human Brain. By JOHN PFEIFFER. Pp. vii + 273. London: Victor Gollancz Ltd. 1955. 16*s*.

This book is written by the Science Director of the Columbia Broadcasting System and President-elect of the Science Writers Association. It is essentially a work of popular exposition. Its material is presented with brisk assurance, and, in general, with clarity, but necessarily at second-hand for its main facts. No extensive scientific vocabulary is required, but some familiarity with North American colloquial speech would be a help. 'Jibe', for instance, appears in an unfamiliar sense. Well-known authorities on the various topics have been consulted, in many cases personally. Amongst them one pauses a little before recognizing 'Edgar D. Adrian'. Maximal use is made of the personal anecdote, the abstracted case history, the actuality description, and reference to matters of current general interest, and prejudice. These last are naturally better adapted to an American public than a British, and emphasize the specificity to which the popular approach is liable, in contrast to more austere methods.

All these devices for sustaining the interest of the uninformed are put to good didactic use, and an eminently readable and informative book has been produced.

The chief reservation in one's praise would be some anxiety lest the production of a simplified account of experimental work in progress should leave an unjustified impression of certainty in the ordinary reader; but this risk is inevitable if the doings of the experts are to be interpreted to the general public, and is probably preferable to total ignorance.

The author is least happy in the more psychological sections. 'Memory and the Higher Faculties' get sixteen pages and rather confused treatment. The mental changes with ageing are presented in a hesitating and inconclusive way. This contrast in lucidity with the rest of the book is presumably the fault of the primary authorities; or perhaps it would be fairer to say that it indicates that this field is the least ready for dogmatic treatment.

J. A. L. NAUGHTON

Management of Addictions. Edited by E. PODOLSKY. New York: Philosophical Library Inc. 1955. Pp. 413. $7.50.

This book will be found most useful by medical practitioners having to deal with drug addicts on whom previous methods of therapy have failed. The greater part deals with the treatment of alcohol addiction, and some thirty authors with first-hand experience describe the wide range of methods available and attempt to evaluate their results. One is struck by the preponderance of therapies involving hormones and drugs, while the once popular use of vitamins is barely mentioned. Among other drugs of addiction discussed barbiturates and 'benzedrine' are included side by side with the more spectacular morphine and hashish.

Psychologists will be interested in the relative contributions of physiological and psychological factors to the development of addictions—a problem which is approached from various points of view including the psycho-analytic—and in the accounts of applications of the more psychological methods of treatment. Among these are group psychotherapy, hypnotic suggestion, and various forms of 'conditioned reflex treatment' in which something nasty is associated with the addicting agent. A good deal of case history material is given, much of it, however, too superficial to throw useful light on the kind of personality which prefers drug addiction to other means of gratification. What, for example, is one to make of isolated cases like that of the patient reported to have turned alcoholic on discovering that he had lost the ability to 'reach to ecstasy' while playing his saxophone?

The book is in the form of thirty-five separate essays of somewhat uneven quality and with inevitably a certain amount of overlap between them, though most of them are to be commended for ending with a short summary and a list of references. A subject index would have made the contents still more accessible.

HANNAH STEINBERG

VOLUME XLVII MAY 1956 PART 2

CHANGING SCENE

BY F. C. BARTLETT

British Psychological Society Presidential Address, March 1951

The late Prof. Beatrice Edgell (1947) told how the British Psychological Society began its existence at an informal small meeting in London on 24 October 1901. This, therefore, is our Jubilee Year, and it seemed to me that perhaps the most fitting thing that I could try to do this afternoon would be to go back over some of the story of these fifty years of growth and development and to tell the tale of a changing scene. There are indeed, as I am well aware, others who have a greater right to do this than I can have, because their relations with our Society began earlier and have been closer than mine. Sir Cyril Burt, Prof. Pear, Prof. Flugel, Prof. Valentine, Dr E. O. Lewis, I believe the Society's next President, Dr William Brown, and no doubt others as well, were all active members before me, and I owe them and tender them all some apology for my choice of a subject which they could all deal with more fully and more intimately. But it seemed that it would be wrong to ignore a great occasion, and I will attempt to talk in the main only of things that I remember, and people whom I have myself known.

On that day in October 1901 I was just beyond my fifteenth birthday. I cannot recall whether I had ever as yet heard the word 'psychology', but if I had, certainly it had never entered into my wildest dreams that this subject would play any large part in my own life. I was in fact at that time very slowly struggling back to health again after an exceedingly serious illness, and the doctors had prescribed for me, as, later, I heard that they had under not very dissimilar circumstances prescribed for Prof. James Ward, a life in the open air. There I was, far away in the depths of the country, at the north end of the Cotswolds, learning something about the habits of birds and beasts, exploring ancient and overgrown roadways, walking far in all weathers, and, as strength returned, playing every game I could get. Nothing seemed less likely than that I should have a life of study and experiment. And indeed, looking back now, it seems to have been the merest chance remark that sent me to a bookshop a few years later to order a book about logic, and then not much after that, my first two books of psychology: Stout's *Manual*, and the old red-covered first edition of C. S. Myers's *Text-Book of Experimental Psychology*. The die was cast, but if I am to be quite truthful, I must confess that I did not then recognize the significance of the pattern. Of course, I thought that psychology, as it was presented to me in these two famous volumes, was full of interest, and I did a lot of amateur experiments, cutting strips of coloured paper out of pattern books, building up some bits of home-made apparatus, and no doubt bothering and confounding my relatives and friends with a lot of odd questions. But I thought also that there were plenty of other things that called to a young man's fancy.

All this time the British Psychological Society was continuing its small and disputatious meetings that I was to read and hear a lot about later on. How many a time I have regretted that I did not hear W. H. R. Rivers backing Hering against Helmholtz, Myers weightily holding the balance, Ward getting very worked up about the nature of 'black', and McDougall expounding what I still think to have been the best experiments he ever did, his earlier Cambridge work on colour vision. It was not in fact until about nine, or perhaps ten, years after the beginning that I went up to Cambridge, met Dr E. O. Lewis and Dr Robert Rusk, both of whom treated me with infinite kindness, heard from them stories of the great men among our founders and builders, and then on one, to me, never to be forgotten day, encountered G. F. Stout at a tea party.

It must, I think, have been towards the end of a May Term in Cambridge, for I remember how the sunshine was streaming in through french windows leading to a somewhat enclosed garden, and it was exceedingly hot. There was an unfortunate lady who was mostly silent and obviously nervous. At length she ventured to remark: 'I do like my cup of tea. First it makes you hot and then it makes you cool.' At this Stout exploded with terrific violence. 'I never heard such rubbish', he said. 'First hot and then cold! Nonsense! Absurd! Ridiculous!'

The poor well-meaning lady subsided into complete silence. I was sorry for her. After all, she only said the right thing in a wrong way.

Stout asked me what I was going to do, and then, probably for the first time, I said suddenly that I thought I should try to teach psychology. 'It can't be done,' he said. 'Nobody can teach psychology. It can be learned. It can't be taught.'

Then I thought he was wrong. Now I know what he meant, and I think he was not far off the mark.

The reason I met Stout first, before any of the others, known by name at least to us all, who had more to do with our Society—though he was, of course, an original member and an Honorary Member for many years—was that I lived a little more than a year in Cambridge before I joined St John's College. I went to St John's principally because W. H. R. Rivers was there, and I was being torn between psychology and anthropology, with a bit of a lurking desire for medicine in the background.

This was already after that fateful division of pathways which took place during or following the Torres Straits Expedition, and probably shaped the early history of the Society more decisively than any other single event. This was the division which deflected Myers finally into psychology and Rivers, for many years, away from psychology and into anthropology.

I can bring back to mind very clearly the first time I met Rivers. Already he had reduced the large, somewhat military looking moustache which adorns his earlier photographs. He seemed to me tall, spare, sharp-shouldered, full of nervous though quiet energy. He sat on a sofa in a large room untidy with books and papers. A College 'gyp' brought up tea, two or three bits of bread and butter, and some very sawdusty Madeira cake. He ate nothing and drank only milk and water. He told me then, and many times later, that if I wanted to study anthropology, or, I gathered, do anything else that was any good, I must first spend many hours in the psychological laboratory learning the psycho-physical methods. I did.

Let me now again pay my tribute to C. S. Myers, and to Sir Cyril Burt. Myers was then Director of Cambridge experimental psychology, and Burt was his assistant, who took charge of the practical classes, and ran them with a most delightful mixture of humanity and scientific precision. I doubt if we can have been a very rewarding class. Privately we grumbled and groused like anything. We vowed we would lift no more weights, learn no more nonsense syllables, strike no more tuning forks, cross out no more e's. Colour wheels were more fun. We threw ourselves at the dynamometer and the ergograph in the hope of beating one another or smashing the apparatus. Optical illusions were temporarily attractive, but we got a bit tired of the Müller-Lyer. Everything that could be was in a strict pattern of psychological method, and now I know it was greatly to our good. But how sweetly we escaped to tachistoscopic experiments on perception and to Burt's own early intelligence tests!

Sometimes now I take down my first experimental note-books and then I realize most clearly of all how changed is the scene since that time forty and more years ago. We worked in the main with the stock-in-trade of Helmholtz and Hering, of Wundt, of Blix, Goldscheider and Von Frey, of G. E. Müller, of Kraepelin. There were a few refreshing excursions into the fields of the Würzburg school, a little of Wilhelm Stern, and a little, a very little, of the new psychology of mental tests, and of Jung's form of word associations. It was Germans, Germans all the way, and if we were going to stick to psychology then to Germany sooner or later we must all surely go.

There were several difficulties that I am afraid are still apt to beset the student of experimental psychology, though I hope less acutely than they did us. When we left the laboratory we promptly dropped it all. What had it to do with our daily life? It was indeed a laboratory game, boring in parts, engrossing in others, but just a laboratory game. And it was not very easy to link up the experiments with the general teaching. When Myers lectured or Burt talked to us it was less difficult. They gave us the classical experimental backgrounds and found time for reflexions upon real life. But when we went to lectures by James Ward and Prof. Dawes Hicks, it was like journeying to a far country.

Ward's first class of the year was a bit of an ordeal. He would come in, suitably late, struggling into an academic gown as he came. He would sit down and take us all in, and then he would go round the class one by one. 'You there. What have you come for?' It did not matter much what we said. He would make it very clear that we were probably wasting our time and certainly wasting his. 'I don't suppose you'll understand anything', he would say. But when, after this, he talked, with great informality and charm, he held us all entranced, whether we understood it (and speaking for myself I frequently did not) or not. Sometimes he asked us for questions, and if we put any he would say, with intense patience, that we had missed all the points which he had most clearly made, and if we did not he would say that anybody who had understood anything would have masses of questions to ask. James Ward became, and I am very happy to be able to say still remains, one of my tremendous heroes. He had done most of his revolutionary work for psychology before this Society was started. Later he spoke to me often about the Society and the people who were shaping its future. Still more he spoke of the *Journal*, which I gathered had been not too bad in its first two years but afterwards had fallen off sadly. He had been one of the original guarantors, and he was mightily afraid that he would lose his money, but I am glad to say that he did not. He was sorely troubled about contemporary developments. Outside of the small range which he would never admit to be more than 'psycho-physics', and a few experiments on memory, he thought most experimental work was trivial, and in a laboratory he was an unhappy man. He knew more than most people realized about the tremendous things that were happening to clinical psychology in Vienna, but he called them 'mad'.

It is, of course, not psychology alone that has changed, but the whole pace and character of social life. The two things have run together as it is inevitable that they should. In the University everybody was more at leisure. There was far less teaching. The only sense of urgency was internal to whatever subject was being studied. All our professors and many of our lecturers entertained us to lunch at least once a term and kept open house at Sunday tea-time. I suppose the food was nothing out of the way then, but now it makes one's mouth water only to remember it.

And the meetings of this Society: they were mostly on Saturday afternoons and in London, as they are now. They were small and the company rather highly selected in an academic sense. The discussion was generally lively and there was Shand's dinner to follow, a delight to the flesh and the mind. I suppose there are few people to remember Alexander Shand now; tall, perhaps somewhat gaunt, white haired when I knew him and almost always in a spick and span navy blue suit. His interests ranged widely wherever a magnificent literary imagination took him. He seemed to have read everything that was worth while and a lot besides. To listen to his psychological discussions was a great treat, but a greater still to sit by him at dinner.

Into this world, cultivated, rather satisfied, well fed and sociable, and preoccupied with its learned problems, came the threat and then the reality of war. The Society still continued to meet, though less frequently, and generally without the dinner. Soon all of us were absorbed, in one way and another, into the great struggle. I cannot attempt to tell the story in detail but only to pick out those things that were happening which were destined to change the British Psychological Society and the whole teaching and practice of psychology in fundamental ways. I think there were three of these. Two may be called new, but the third was already well under way in this country under the inspiration of Cyril Burt and Prof. Spearman and others.

First, whether it was by accident or by foresight, experimental psychology got tied up with technical physical advances. This was due in the main to the influence of Myers and of Prof. Pear. Submarines were already being attacked by underwater methods of sound transmission, and in this a body known as the Lancashire Anti-Submarine Committee, led by the late Lord Rutherford, took a somewhat emphatic lead. But it soon became clear that not everybody could operate the instruments which were invented and constructed, and Myers and Pear together were able to see that a group of us had the chance to develop what was probably the first really organized effort of team personnel selection, in a technical sense.

Secondly, there was the vast increase of neurotic disorders among the fighting men of the First World War. Myers, Rivers, McDougall, Dr William Brown, Elliot Smith and again Prof. Pear began to show what psychology could do to meet this menace to health and efficiency.

Thirdly,—but perhaps the best way to tell of this will be by a brief story. I remember well how, towards the end of the War, Dr W. V. Bingham came over from America. He brought with him copies of the American Army general intelligence test, and I met him on Saturday afternoon at University College. I am not quite sure who else was there, but certainly there were Myers, and Spearman, and Prof. Edgell, and perhaps Burt. Anyway, Bingham gave us all the tests and as far as I can remember not many of us would have made the grade. We felt a bit doubtful about them, but it was clear enough that they had come in some form or other to stay, and we must be prepared to do something about it. After all, Spearman and Burt particularly had little enough to learn in this field.

It seemed then and for many years following that these three developments—the technological link with the physical sciences, the strong urge to base everything in psychology on clinical analysis, and the intelligence test movement with its active association with statistics—could not well be brought together. Each represented an approach which appeared somehow a bit distasteful, or at least foreign to anybody who preferred to adopt one of the others. But during that same period another development took place in this country, the fundamental implications of which have not yet, I think, been fully realized. It was due to Sir Henry Head and Dr Gordon Holmes.

Head is not easy to put into a word portrait. He was a man of most brilliant parts, with a glancing intelligence that shot off in all directions, with remarkable histrionic powers, equally at home in literary, artistic and scientific circles. More than most people he combined a terrific capacity for rather impatient experiment with an amazingly fruitful power of guessing. He was a large man, florid to look at in those days, flamboyant, a bit of a buccaneer. He could easily have been mistaken for an artist or a poet, and, indeed, he had published a book of poetry which many thought very nearly first rate. Once when he was having dessert after a feast at St John's College in the Combination Room, where the long table was lit by tall candles in their gleaming silver stands, completely carried away and waving his arms in magnificent gesture he declaimed, 'The table lit by a thousand candles'. Rivers's old 'gyp' was passing by at the time. He stopped, bent over: 'Two hundred and fifty seven, sir', he said, 'to be exact.'

It was during the First World War that Head began effectively his work on aphasia, famous at the time, though now, I am sorry to say, apparently less regarded. Most of his patients were soldiers with gun-shot wounds in the head but otherwise healthy. His method was a highly original combination of experiment, test and clinical analysis and assessment. From time to time Head used to invite me to go up to London for discussion, the discussion being in fact almost all on one side. He put me through all his tests and experiments, and I must say that he made it hard to get any results different from the ones he knew to be right. But never mind, they were genuine experiments, and in his work the three main developments of these war years came together.

Prof. Edgell recorded the factual history of the great changes that came to this Society in the years between the two Great Wars. What I will try to do is to tell the story more from the inside and in terms of some of the principal personalities concerned.

W. H. R. Rivers was back with a bang right in the middle of the psychological scene. All those reclusive habits which, when I first knew him, he seemed to be hanging on to with grim determination, he gaily flung to the winds. He knew everybody and went everywhere. He produced paper after paper, all of them theoretical, all sweeping and dogmatic, and all of them written out in handwriting that was almost completely unintelligible to anybody except himself, but in language and with ideas that everybody thought he could understand. By bent of mind and character he was really no organizer of people—though in his own way he tried his hand at this quite a lot in these days—but an untiring organizer of ideas. He had a vast and humanizing influence on the intellectual outlook and preoccupations of the Society, but none at all, save only indirectly, upon its shape and outward constitution.

Yet the indirect influence was great enough. It came through C. S. Myers, who was a little younger and then in the habit of discussing all his schemes with Rivers. Both of them in those days saw psychology finding its place in every form of human activity—medical, industrial, technical, political, literary and artistic, and educational—and gathering into its fold all manner of people of intelligence and good will. They shared the same vision, but whereas Rivers passed it on to others in a personal way, Myers had the patience, the skill and the genius to give it an institutional form.

In 1922 there was an International Conference of Psychology at Oxford. Anybody who cares to look up the published record of the Proceedings will see how much it forecasted the impending expansion both of this Society and of psychological interests generally in this country. Myers was its President and was in fact behind the scenes in every part of its organization. Everything that became a live sectional interest of the Society had its place there: the general and experimental, the industrial, the medical, the social and the aesthetic. The manner of it was also significant. The appeal was wide and unlimited, but the leading performers were in fact selected unobtrusively but with great tact and skill. The German dominance had gone. It is true that a small band of Germans were there and made a profound impression. But they were themselves in revolt against the classic German tradition; they were Wertheimer and Kohler and Koffka. If one looks at this Conference as a whole it is easy to see that its main trend was either wholly away from the academic and intellectualized psychology of the past, or at least towards an effort to humanize all the rather arid traditions.

In these days Myers was restless, unhappy about the state and status of psychology but unswervingly loyal to what he regarded as its true claims, and full of plans. The British Psychological Society must grow. It had to grow in order to maintain its *Journal*, but apart from that it must grow to match the rapidly expanding interests in animal and human behaviour, which, as he saw before most people, must profoundly modify psychological study and research, if not within our control, then outside of it and probably to its own cost. Of course, he was not alone. Spearman, Burt, Pear, Drever, Prof. Edgell, Flugel and a good many others played their part as well, but I think we would all acknowledge that he was the chief architect.

This was the period during which the three principal lines of psychological interest which grew up during the First World War proceeded in extremely rapid, but in the main, separate, growth. Above all, maybe, they were the years during which almost every psychologist proclaimed himself an anti-intellectualist. Feeling, emotion and instinct were the master keys, and in many quarters they were melted down into a single master key in the pattern provided by the genius of Sigmund Freud. Almost equally they were the years of tests, and selection by test for all manner of practical activities, and of the statistical analysis of test results which, oddly enough in spite of all clinical bias, seemed in many ways to be holding fast to an intellectualist basis. Both of these movements naturally took many psychologists, especially in America, into a world of society, and methods of social investigation were rapidly shaped which borrowed both from the clinical and from the test pattern of approach. All this time I think experiment proper in psychology was a little unhappy, alternatively becoming immersed in more and more detail of the classical type, and trying to break out of the laboratory but uncertain how it could do so without doing violence to the established principals of scientific method.

All this was bound to be reflected in the proceedings and organization of the Society. It grew rapidly in size, as everybody knows, but I fancy that its sectional form tended rather to divide than to unite its interests, for each section shared in the movement common now to all science and most knowledge and became more and more specialized. Professional problems and questions of status and reward, unknown to the Society in the earlier days, became inevitably more insistent, and however much those of us born in easier times were tempted to disregard them, we could not deny that they are important and must somehow be solved.

Then came the Second World War. We are too near to this as yet to assess fully its impact upon psychology and the Society. But one thing seems to me to be already clear enough. It is that the technical achievements upon which the conduct of almost all military operations now depend helped both to re-establish experiment again as the method of scientific advance without which no sure progress can be made, and also to bring the laboratory into closer touch than ever before with the world outside. It now seems that the emotionalism of the twenties and thirties was as one-sided and insufficient as the intellectualism that preceded it. It even seems that social problems cannot be finally or satisfactorily solved by just thinking about them and adumbrating general ideas and schemes. We are perhaps learning how to combine the clinical with the experimental; we are perhaps coming to see that no matter how sectional and specialized our particular interests may be they must come together in a basic methodology which can unite explorers in all the fields of human knowledge.

What all this may mean for the British Psychological Society nobody can yet tell. But when I try to compare especially the psychological laboratory of to-day with the environment in which I began to work, I see a tremendous change of scene. The sense of leisure has gone but with it also, to a large degree, though not yet entirely, a sense of artificiality. Psychology, especially on the experimental side, is no longer just hoping to be a science based upon a little anatomy and physiology and a lot of speculation. It has its widely acknowledged link with all the other sciences and with all the practical activities of man, individual and social. It demands an equipment far greater, more specialized, and at the same time more all round, than we ever had, and it offers chances of which we never dreamed. But above everything else, as I see it, the old puppet subjects of the early German psychologists have had their time. They have played many a good play. Their strings are broken. Their commendatory epitaphs can be written and they can be put away in their boxes. In their places we will have real people.

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THE BOUNDARIES BETWEEN THE PERSON AND THE WORLD*

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In contrast to the widespread opinion that organism and environment (or person and world) can be sharply distinguished, the thesis is propounded that the one often melts into the other. Data from physiology, genetics, behaviour psychology, the psychology of immediate experience, social psychology and parapsychology all suggest that the 'gradient' from person to world may be either 'steep' or very gradual.

I. INTRODUCTION

In ancient Greece and in ancient India, and throughout the history of the two civilizations which have grown from them, there have been several perennial answers to the questions of the relation of the person to his world. While psychology gathers new data and develops new viewpoints, it still seems to revolve mainly within the classical orbit set by these ancient philosophies, rather than attempting in the name of science basic and fresh formulations in relation to this problem. A central aspect to be treated here is the question of boundaries—spatial, temporal, functional and conceptual—which mark off the realities and events called personal from those called extra-personal. To suggest the range of possible ways in which we may conceptualize the relation of the person to the world may perhaps help to make clear what we do not at present understand about the nature of persons, and may help to focus experimental, clinical, comparative and other research procedures toward the attainment of a clearer and more workable formulation of the possibilities.

The boundaries between person and world may be viewed in terms of physical, biological, psychological and social science.

II. PHYSICAL AND BIOLOGICAL BOUNDARIES

From Democritus to Schrödinger (1945) the physical view suggests that what is inside man is essentially like what is outside; he is a bit of the cosmos, duplicating it in substance, frequently also in structure.

The world as envisaged, for example, by Democritus, consists of particles, some of which are bunched in the form of organic beings, and some of the particles within such sentient beings make up the 'mind' of the person. From this conception, through Lucretius, Hobbes, La Mettrie, and the monistic systems of to-day, an essential identity of thought can be observed. From this point of view, there is a clear and obvious boundary between person and non-person, namely, the capsule, the envelope, the spatial boundary, essentially the skin of the living individual. At times, the monist espousing such a view glimpses a larger and more complex relationship, as when La Mettrie, for example, compares man to fire, the process of respiration involving a subtle and fluid interaction between person and world. Though the cosmos permits momentary isolation of small portions of itself, biochemistry has made clear, for example, that in vital processes like respiration it is meaningless to define a sharp boundary. Thus, in Henderson's 'nomogram' of the blood (1917), the living system within the skin is not sharply separable from the non-living system in which it is bathed; the air in the red-blood cells can be regarded either as part of us or as part of our environment, indifferently (Northrup, 1931). The gradient between man and world is a gentle one indeed.

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Biological research nevertheless forces our attention to an apparent paradox: the fact that the organism is ultimately inseparable from its environment means that its very existence depends upon its obedience to principles quite different from those which generally obtain outside it. In an 'open system', for example, specific processes, such as the maintenance of homeostatic balance, prevent the irruption of the outer form of order into the protected inner realm within which a fundamentally different kind of order prevails. As C. D. Broad (1925) phrases the matter, we find, in contrast to the principle of substantive vitalism, the operation of 'emergent vitalism', a conception according to which the same material particles which are found in the universe at large are found under conditions of such organization and at a level of such emergence as makes vital phenomena essentially different from those occurring in the outer world. We may thus, if we choose, avoid dualism of substance, yet provide dualism of functions.

Thus, despite this conception of homogeneity there are constant attempts to develop some plausible basis for the fundamental struggle of the individual against the world. The conception of homeostatic balance enables us to envisage ways in which particles essentially like those of the cosmos at large may, nevertheless, as it were, fight against the disintegrating forces of the cosmos, maintaining for a while their own tightly structured internal coherence. Lifting this conception to the cultural and ideal sphere, we have the view propounded by Thomas Huxley's *Evolution and Ethics* (1894), and Bertrand Russell's *The Free Man's Worship* (1923), in which the value system of human beings negates, opposes and stands in stark opposition to the valuelessness and impersonality of the world.

But it will surely not suffice for science to declare only that we are both one with the environment and also embattled against it. The empirical issue is to ascertain just when and how far Henderson's conception of the *gentle gradient* holds good, when and how far the *sharp gradient* of homeostatic isolation prevails, and just when and how our relative independence of the environment is to be defined. Leeper (1954) points out that the term gradient is used here for no single physical continuum. Rather, I refer here to the continuum of *degrees of interdependence*. Ultimately, I suppose, our thinking about degrees of interdependence may become strictly quantitative, in which case we could refer to covariation.

III. PSYCHOLOGICAL BOUNDARIES

Turning now to the viewpoint of psychology, we find the gradient between person and world ordinarily treated as very steep. We speak of individual and environment as two utterly distinct things, with a sharply defined boundary. Our diagrams and our conceptual schemata show no region of transition; everything is either inside or outside the organism. Yet despite all this, contemporary psychology offers at least three other possible approaches.

First, the Gestalt conception of inner structure, such as the structure of thought or the structure of personality, in contrast with a lower degree of order in the environment, reveals a parallel with the sharp gradient of homeostatic balance.

The concept of isomorphism is an effort to give the individual a structure and an autonomy, while providing resonance of inner to outer reality. The term *isomorphism* has been used in two senses: first, to denote the correspondence of mental structure with the structure of neural response; and secondly, to denote the correspondence of inner structure with environmental structure. It is to the latter that I refer. Isomorphism, indeed, is a special case in which inner structure mirrors outer structure, not through capitulation of the inner to the outer, but through their capacity for attunement. Compare here the the biophysicist, Selig Hecht, with the Gestaltist, Arnheim (1951). Hecht (date unknown) is quoted as saying: 'The organism does not adapt itself to the environment; the environment adapts the organism to itself.' For Arnheim, on the contrary, isomorphism is that structural correspondence between the physical environment and the psycho-physical processes in the person which makes adequate reaction to the world possible. In relation to music he writes of 'the structural kinship between expressive effects and specific patterns of rhythm, pitch, harmony, timbre, or volume'.

A second concept characteristic of modern psychology presupposes formal independence of person from world, combined with absolute dependence through the ceaseless give-and-take tennis game of acts and signals, the world of cybernetics and of information theory, of Brunswik's navigation through a sea of uncertainties (1943), Leeper's complex contacts with heterogeneous reality (1954), Sears' dyadic functions (1951)—a world where reality is reciprocity, not the world of events-within-the-individual-person which our self-reliant grandparents knew.

It is not ordinarily noted here that in many such cases the interdependence of part of organism no. 1, say its vocal cords, and a part of organism no. 2, say its ears, is very much closer than the interdependence of most parts of organism no. 1. If this be mystical, make the most of it. Functional interdependence is what we are now studying, and the lines of functional interdependence do not neatly run in closed circuits inside one individual.

The third psychological conception goes even further. By all means the most original and radical of the modern efforts to transcend these traditional dichotomies, is the field theory of Kurt Lewin (1935). Incomplete and in some ways self-contradictory at the time of his death, this was, nevertheless, a brilliant adaptation of the general scientific conception of the field as developed first in physics and then in biology, transported into pyschology to indicate the relative inseparability of person and world. As a matter of fact, it is only in Lewin that one refuses to accept the ancient question: Are we one with the world or isolated from it? Neither, says Lewin, and proceeds to show how those aspects of a response which are properly called personal and those aspects which are properly anchored in the environment come to meet one another in one organized field dynamic. In the 'life-space' of Lewin there need be no fixed division into outer and inner.

In some types of field theory, sketched, for example, by Angyal (1941) and by Sullivan (1953), a unified world of person and environment yields at some points a gradient as gentle as that of Henderson. I have elsewhere tried to show that in many empirical problems like the preparation of a life history, the field conception, with its fusion of outer and inner, notably improves our powers of explanation and prediction. While all concepts of the nature of the individual encounter deep-seated resistance, the notion that we tend to melt into our environment encounters a resistance which is perhaps proportional to our culturally engrained conviction that there *must* be something which is utterly our own.

IV. SELFHOOD

Up to this point we have been able to use the terms *person*, *organism* and *individual* more or less interchangeably. This is because we have been considering ways of attempting to view objectively the relations of organism and environment. There is, however, another issue which may perhaps be of interest in connexion with the problem of objective boundaries, namely, the problem of the way in which the barrier is viewed by the experiencing individual. This is the problem of our awareness of our own individuality, commonly called the consciousness of self. Classically, throughout both Indian and Western traditions, the thing known as the self, the observed me, develops like an object of experience, comparable in all fundamental respects to the development of other percepts. The newborn have no clear awareness of self; but gradually through the observation of body surfaces, muscular contractions, the sound of the voice, cathexis upon these physical entities and constant comparison of them with other entities observed in other persons, an empirical self is woven and given abiding structure. This self may undergo transformations and elaborations, but it represents a relative continuity from the cradle to the grave, is given a name, is given a social position and sense of responsibility, and around it in time cluster the goals, values, norms and ideals which give meaningful purposive continuity to life. It is fundamental to most such theories that the self becomes a loved object, Freud's narcissism being the clearest of the Western formulations of such self-love.

There is, however, constant reciprocity between the image of the self and the image of others; and this means that fundamentally, from the point of view of an empirical psychology, the contours of the self are often blurred, and the distinction between the self and non-self indistinct. This is fully recognized, both in associationist doctrine, in the empirical studies of the school of Piaget, and in the psychoanalytic system. As a matter of fact, the question arises, as one studies both Piaget and Freud, whether the self ever does become sharply separated from the perceived selves of other individuals and consequently whether a sharp delineation of the self is ever achieved. One might set up a hypothetical dissociation so profound that selfhood is separated by a deep gulf from all else in the world. This is hard to imagine in view of the endless social commerce of selves which have to be perceived as phases of a process of social interaction in a context of wholeness. Yet in certain types of mental illness, this extreme separation can be observed or hypothecated, while at the other extreme other types of illness involve an incapacity to separate, even relatively, the regions definable as self. Empirically, then, there are *degrees* of independence of self from non-self.

There exist, however, in all known societies a great many phenomena of awareness in which self is not distinctly marked off or in which it has properties quite different from the self as we know it. There even appear to be some completely selfless experiences existing momentarily, and experiences of complete immolation or identification in group or cosmic processes. Some of these experiences have been studied in early childhood, through the clinical methods of modern psychiatry, through the methods of ethnology, and occasionally through the methods of experimental psychology, especially in the case of depersonalized hypnotic states. Such investigation shows that the sharp isolation of the individual is merely one special case of consciousness, like Euclidian geometry among non-Euclidian systems. There is, moreover, some experimental material on the conditions which lead to such impersonal states and which throw light on their broader significance. There is, of course, an abundant opportunity here for self-deception, and it would be ridiculous to maintain that the person's experience of boundaries between self and non-self is a safe guide to the real situation. He may be a psychotic whose illness consists in large part of his inability to accept the real boundaries. But if the fusion of selves is experienced by some persons as a reality, this constitutes a psychological problem which may merit study, along with the problem of the nature of the objective boundaries.

This soon leads over a bridge which to many will appear to be perilous indeed. For selfhood is seen from all these vantage points to be capable of only a relative definition. It represents a reality which is only partially detachable from a flux of experience not sharply organized around the self. This re-opens some issues raised in ancient Greece and in ancient India regarding the real separation of one person from another. This question remainds one of John Donne's words: 'No man is an island.' Such a conception, developed by mystics in both the Eastern and Western traditions, has pointed to a type of psychological reality which is not anchored in any single self. It has insisted that there is not only the reciprocity of self and social environment, but at times a true and intimate fusion of selves, of personal identities. This view has usually been conceived in religious terms through identification with a Deity, or as in the Greek mystery religions, as identity of the fellow worshippers in their identification with the Deity. There is, however, much more here than a system of religious and philosophical possibilities. There are challenging psychological problems which have been posed by studies of sleep, trance, hypnosis, ecstasy, the phenomena of 'possession' described in the ethnologists' and historians' records. The empirical problem of the degree to which selfhood can disappear leads on into the problem of the interaction between two or more selves in which the isolating sense of individuality tends to be lost in a sense of communion.

Please let me repeat for emphasis: I do not mean to suggest that the individual's sheer belief that he is dependent on others, or independent of others, should be taken as anything more than just a belief. He may, of course, actually be more dependent on others and on the environment than he thinks he is, or he may be less dependent. My point, rather, is that the problem of dividing the world into experienced self and non-self is a problem that needs to be pursued conjointly with the problem of the nature of the objective biological gradient.

It might be thought that an appeal for a study of conditions of selfhood with very fluid and indistinct boundaries is essentially an appeal for a study of queer, bizarre or abnormal states. On the contrary, it seems very inappropriate to use normative concepts, terms of evaluation and derogation, when dealing with states of consciousness about which we know so little, and which in the nature of the case appear alien to our more customary ways of thinking. William James remarked: 'The world of our present consciousness is only one out of many worlds of consciousness that exist.' I put to you, therefore, the question: Is it better for psychology to finish in all completeness the study of one kind of awareness, or to strive for an openness towards the possibility of areas of experience as yet unexplored?

I believe that our reluctance to investigate these phenomena is connected prominently with the vigorous individualism of Western history, and connected likewise with the feeling that our long cherished emancipation from philosophy and religion will somehow exempt us from coping with problems of this order of complexity. As a matter of fact, the same motives and the fear of losing caste among positivist scientists has interfered on a huge scale, even with the investigation of relatively simple and objective laboratory phenomena. Such laboratory phenomena, for example, are now available in the field of parapsychology, meeting the challenge for strict experimental control. A huge range of empirical materials is now at hand, interconnected by a very considerable body of coherent systematic conceptualization. Most of this conceptualization is unknown to psychologists, indeed even the empirical material is largely unknown One often hears the remark that one might begin to believe in the authenticity of the many experiments in telepathy to which no objection has been pointed out, if there were some sort of coherent theory regarding it. Yet several coherent theories have already been developed in intimate relation with the experimental facts, and progress in development of repeatability of experiments goes hand and hand with clarification of the conceptual systems. Psychologists are, for the most part, unfamiliar with this material, and, as scientists, I think one might say quite literally, afraid that there might be some truth or some value in these lines of inquiry. Science can seldom afford to be afraid of investigation, and in this instance, as in so many others in the history of science, fear can actually cripple the types of investigation which would give true unity and resolution to what have proved otherwise to be insoluble problems.

In the experiments of Soal and his collaborators (Soal & Goldney, 1943; Soal & Bateman, 1954), it has been possible over a span of several years to observe meaningful variation in telepathic scoring level between subjects working in different rooms, with independent observers in both rooms, with randomized stimulus materials and elaborately careful and cautious statistical treatment. The phenomena were not only in conformity with reasonable psychological hypotheses regarding the dynamics of the process, but exemplified the specific point here suggested, namely, the interpersonal nature of the telepathic phenomena. In some experimental series, two separate individuals working independently each possessed some information about the stimulus material, but neither one knew its whole character. A third person, *C*, the sensitive subject, was able to piece together the material, so that this mind *C* actually represented, at the time, an integration of the contents of three minds—*A*'s, *B*'s, and his own. I ask only that you take this as a challenge to read or to think. It is perhaps of interest that many of the phenomena suggested by unconventional ways of looking at the person raise essentially the same kind of question, namely, the question where one person begins and another ends.

V. SOCIAL BOUNDARIES

Finally, in the social science approach we have been pushed by more and more evidence in recent years to admit the profound interdependence of members of society, the full membership character of individuals in their group, and the reality of processes which are fully interpersonal, not simply the accretion of the behaviour of distinct individuals. Again societies differ in the degree of autonomy allowed to the individual, with autonomy varying from great to little, but never suggesting the reality of any wholly self-sufficient person, any more than psychology, biology or physics could allow such absolute self-sufficiency.

How did we manage to forget so much of this in erecting a pedestal upon which the individual was to stand—poor Simeon Stylites, alone in his elevation above the group?

We forgot, perhaps, because we are parts of the Western conception of individuality, made razor-sharp by the Commercial and Industrial Revolutions. We grow to-day in actuality more and more dependent upon one another, but the rugged individualism of a tradition is still spread all over our psychology. Is it really necessary to give the Western conception of individuality not only the right of way, but actually the sole berth in the establishment of psychological science? Is there a possibility that by greater openness, we could recognize that in Western psychology we have only one of an infinite variety of possible relations of self to world?

VI. THE NEED FOR RESEARCH

Not, of course, that we are fully consistent. Perhaps we might conclude that some aspects of persons are relatively sharply bounded, others the reverse. Paradoxically, we give heroic stature both to the supreme Whitman-like 'individualist' and to him, like Gandhi, who is 'selfless' in devotion to a social goal. In an era in which humanity can hardly expect to survive without fuller knowledge of the biosocial value of individualistic and social impulses, research is needed into the sources both of man's need to isolate himself and of his need to soften his individuality in the presences of his fellow men and of the sea and the stars; we need to understand how his culturally moulded selfhood sometimes leads him to make the gradient seem much *less steep*, sometimes much *steeper*, than it is.

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THE PRINCIPLE OF INDIVIDUATION AND THE CO-ORDINATES OF CONDUCT[*]

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Theories can be of two kinds, 'disciplines of thought' or 'speculative hypotheses'. The 'principle of individuation' provides a discipline of thought capable of embracing the mathematical equation, the dimensions of physics, biological interaction between individual and environment, and the psychosomatic problems encountered in medicine and in so-called para-psychology. It indicates not only the lines along which divergent schools of psychological thought and inquiry have developed; it gives order and clarity to the various interpretations of conduct suggested, and the hypotheses put forward for experimental investigation without which the collection of data becomes confused and unproductive. It also gives the statistical treatment of quantitative studies their appropriate place and function as a means of determining the degree to which any qualities assessed are in fact genuine co-ordinates of conduct and dimensions of personality.

I. INTRODUCTION

My paper is concerned with the problem of formulating the appropriate questions to ask in order to provide an adequate psychological account of human conduct. In a former paper (Raven, 1951) I have tried to show that if applied psychology is to advance, there must be a corresponding advance in psychological theory. Nowhere is this more urgent than in the fields of clinical and abnormal psychology.

We have, I think, to recognize two distinct kinds of theory. We can call the first kind 'pure theories' or 'disciplines of thought'; we can call the second kind 'speculative theories'—that is, 'interpretations' of experience in the light of past events and 'hypotheses' concerning future events which can be verified by observation.

Whereas 'speculative theories' require verification, 'disciplines of thought' concern the processes of reasoning from accepted premises to rational conclusions, if not regardless of facts, at least regardless of all facts outside the universe of discourse agreed to at the outset. Mathematics, for example, is a discipline of thought. As Guilford has pointed out, mathematics is essentially theory without speculation. It is based on the *a priori* premises that one and one shall equal in all respects two, and that any observations of one and one not making exactly two (as happens, for example, in all vital processes) shall be excluded at the outset from the universe of discourse. For the sake of argument it is also agreed that qualitative differences between individuals shall be systematically ignored. In dealing with the psychological problems, the limitations of mathematics are therefore obvious.[†]

In order to give a systematic account of living activity, the psychologist needs to begin a stage earlier in his experience than the physical scientist. As Lewin points out, the psychologist must give a systematic account of the processes of individuation themselves, as well as of the individual entities which emerge from their surroundings, and can subsequently be compared and counted. It follows that for the psychologist to adopt the purely objective discipline of thought appropriate to the physical scientist, is for him a confession of defeat—a confession that he is unable to give a consistent account of what he actually experiences, and that as a result, psychology must remain for him a branch of the physical sciences or of sociology, instead of what it can, and I think must, necessarily be, namely, the science of individuation itself.

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[*] Based on a paper read at the Annual Conference of the British Psychological Society, 1955.

[†] Because we find $fx \rightleftharpoons Fy$, it does not necessarily follow that f and F can be adequately formulated mathematically, i.e. in purely additive terms. Nor does it follow that there is any function ϕx which equals in *all* respects ΦY. Mathematics therefore provides useful symbols, but suggests a misleading discipline of thought.

II. The principle of individuation

Gestalt psychologists following Bergson have shown that we apprehend experience as a whole before we perceive the parts constituting it, and that we go on from the whole to the parts by a process of pragmatic analysis into figure and ground, individual and environment, self and non-self, as interrelated opposites. Normally also, as awareness increases, an experience tends to differentiate into a knowledge of oneself attending to particular characters of the experience, first as interrelated qualities, and finally as individual entities, which can be thought about separately as if existing apart from their surroundings. 'Individuation' may therefore be defined as *the emergence of individual entities, objects and modes of experience and behaviour as systematically organized parts of larger unitary wholes, from which, by a process of differentiation, they acquire individual characteristics.**

It can be seen that the appropriate psychological discipline of thought consists in asking this question: Of all the qualities by which individuals are distinguished one from another, which of them are genuinely opposite in nature, and what, therefore, is the minimum number of antitheses (or 'co-ordinates') required for a systematic account of human conduct?

III. The co-ordinates of conduct

(A) *Introverted-extraverted awareness*

For the purpose of description and assessment, we must obviously begin with the universal tendency of experience to differentiate into subject and object as opposite poles. Even in the physical sciences the illusion of pure objectivity is rapidly thinning, and the part played by the observer is coming more and more into view. We shall never succeed in giving an adequate account of human personality by neglecting either subject or object, for both play decisive roles in any idea we form of ourselves or of the world we live in, and any attempt to ignore either in our anxiety to emphasize the importance of the other must eventually lead to disaster.

Introversion and extraversion are perhaps the two opposite poles of experience most frequently discussed, and the dimension of personality most often assessed in one way or another, either from what a person says or from other ways in which he behaves. Nevertheless, people differ not only in the degree to which their attention appears to be centred in the subjective world of inner reality, or in the objective world of outer reality; they differ also in the degree to which these two antitheses of observation, within and without, reciprocate each other, so that a person achieves his desires in harmony with his surroundings. We need therefore to think of subject and object as a pair of interrelated opposites, differentiating from each other to form an essential co-ordinate of conduct, while from the work of Jung it can be seen that speculative interpretations and testable hypotheses concern the degree to which a person seems aware of his needs and his surroundings, and is able to adapt his behaviour accordingly.

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* The *Oxford Dictionary* defines individuation as 'the processes leading to individual existence'.

Briefly, we ask: How far is a person subjectively conscious or unconscious of his own needs; to what extent is he objectively aware or oblivious of his surroundings, and, as a result, how far does he seem able to act as he desires in sympathy with his environment? In short, how far is a person able to find what he wants?

(B) *Intellectual organization*

Simultaneously with, but nevertheless independently of, and in this sense 'orthogonal' to, the differentiation of experience into individual and environment, there emerge intellectual judgements concerning the relations between them, so that the degree to which a person satisfies his needs in any given environment depends in turn upon his capacity for rational thought, as a result of which whatever he observes acquires a degree of systematic organization. How far systematic organization is a product of intellectual activity itself, or is an intrinsic characteristic of natural phenomena, is a metaphysical question concerning which we need only point out that it is a universal co-ordinate of knowledge. Whether we look inwards to the realm of thought, or outwards to the world of things, as far as we perceive individual entities and conceive relations between them, all knowledge is systematically organized. Differences concern only the level of the systematic organization observed.

As far as theoretical principles are concerned, we already have the contributions of Spearman and Wertheimer, to mention only two, while speculative interpretations and testable hypotheses concern the degree to which a person's conduct manifests intellectual organization.

The question is this: Compared with others, what is a person's present capacity for rational judgement; what abilities has he at his disposal, and to what extent does he use them discriminately; or if he does not, what difficulties and disabilities does he encounter? In short, how far does he act intelligently?

(C) *Affective appreciation*

'Orthogonal' to both the co-ordinate of introverted-extraverted awareness, and also intellectual organization, we have again the co-ordinate of affective appreciation, in the sense that the subjective world of thought and the objective world of things alike tend always to be apprehended as being to some degree 'pleasant' or 'unpleasant', 'attractive' or 'repulsive', 'good' or 'bad', irrespective of the degree to which either is apprehended as systematically organized entities.

Apart from the work of Shand and Scheler, the role of affective appreciation in the genesis of character has been emphasized chiefly by psycho-analysts. Unfortunately, in contrasting the 'pleasure principle' with the 'reality principle', Freud confused standards of personal judgement with standards of social approval, and telescoped pleasant-unpleasant feelings with subject-object relations. As far as direct experience is concerned, affective appreciation remains the basis of value judgements throughout life. The change which takes place is that as maturity is reached, an increasingly high value is placed on social approval, until for many people it is adopted as the highest value of all, and the absolute standard of right and wrong, reality and fantasy.* As a co-ordinate of conduct, the psychological study of value judgements is still in its infancy. At present we can say little more than that goodness and badness form an essential co-ordinate of conduct, determining what we attend to as important, ignore as valueless, or reject as evil; while speculation concerns the degree to which a person's conduct reflects his affective appreciation and critical evaluation of events.

Briefly we ask: What does a person tend to seek, enjoy, and appreciate as good; what does he turn away from, depreciate or fear as bad; in making his judgements what occupies his thoughts, what does he ignore, and what are his standards in assessing events and organizing his conduct, or how far does his conduct reflect a lack of affective appreciation and faulty evaluation of events? In short, what interests him?

(D) *Persistent change*

Finally, whether we consider the inner world of thought or the outer world of things, their systematic organization or their relative value for us, continuous change is seen to be an essential co-ordinate of all experience. When we turn our attention inwards, this continuous flow of change tends to be apprehended as conative activity, or persistent striving towards future ends of action in the light of past experience. When we turn our attention outwards, it tends to be apprehended more as some form of physical energy, or organic function. The more awareness becomes extended in time, so that we recall the past and anticipate the future, the more we form definite ideas of ourselves persisting through change, and endeavouring to achieve future ends of action in the light of past events, so that the past is not merely recalled and repeated, it is also reorganized with respect to future events, and what we are attempting to achieve or become.

Here we have the psychological contributions of McDougall, Piaget and Gardner Murphy, although problems of enduring change with time have so far been dealt with chiefly by people like Bergson, Whitehead and Woodger. Speculative interpretation and hypothesis concern the degree to which a person's conduct shows persistence of determined effort in changing circumstances.

The question is this: How far does a person's conduct at any given time manifest well-defined intentions; and in pursuing his objectives, does he adapt his acquired abilities in ways which steadily develop his own individual integrity through time and change; or does his conduct show excessive fixity or mobility of ideas and rigidity or impulsiveness of action, resulting in fragmentation of thought, autonomous behaviour patterns, and instability of character? In short, is his behaviour self-consistent?

IV. THE THEORY OF OPPOSITES AS A PSYCHOLOGICAL DISCIPLINE OF THOUGHT

It would seem that the principle of individuation, and the pairs of related opposites which tend as a result to emerge as the co-ordinates of thought and behaviour, provide psychology with a consistent series of questions concerning human conduct capable of linking together the present divergent schools of psychological theory and investigation. In dealing with psychosomatic problems in general—even in dealing with the abnormal extension of awareness in time and between individuals encountered in the field of so-called 'para-psychology'—the main value of discipline of thought based on the individuation of experience into related opposites is that it reorientates the questions we ask in a way which makes it possible to answer them more satisfactorily. Instead of *starting* with the assumption that the mind and the body are two distinct entities, and *then* asking how they run parallel, interact, or arise epiphenomenally one out of the other, we begin to ask quite different, and, I suggest, much more profitable questions, both speculatively and also for experimental investigation.

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* Hence the description of 'reality' as 'the fantasies we share with other people'.

ACADEMICALLY, we begin to ask: If a living individual is an evolving form of systematically organized activity, why do we imagine two separate entities, body and mind, distinct from each other, yet related with each other in ways we find it difficult to explain and impossible to agree about? In other words, how do we distinguish mental activity from physical activity, and what do we assume to be the essential differences between them?

From our various ideas of mind and body, it would seem that the different concepts we form depend upon the different qualities of conduct contrasted as opposite in nature. In general, the more a physical body of any kind behaves in such a way that it maintains its own individual identity by *adapting to* environmental changes, the more we think its behaviour expresses mental activity, and that it has a mind or will of its own. Conversely, the more any form of behaviour is observed to endure as an organized form of activity *regardless of* environmental changes, the more we tend to think of it as an organic structure, showing an inertia characteristic of a physical body. As a result we come to think of a *material body*, and to define it with some accuracy, as *an enduring form of organized activity*, while we come to think of *mental activity*, and ultimately define it with some accuracy, as *those changes by which an individual adapts to present situations in relation to its own past and future.*

IN PRACTICE, and particularly in clinical work, the question therefore becomes this: How far does a person's thought and behaviour appear to be an original appreciative response harmoniously related to his surroundings; or how far does it appear to be an established organization of activity, enduring regardless of environmental changes and personal desires. In other words, how far does his conduct appear psychogenetically determined, or determined by his organic structure?

It can be seen that we are dealing not so much with two distinct entities, mind and body, as with two opposite qualities of all natural phenomena, and that the real antithesis lies between nature naturing itself, and nature natured; that is to say, between the responsive, selective, organizing, and in this sense creative aspect of natural phenomena, and the resulting forms of systematically organized activity retained and propagated through time, space and experience.

V. CONCLUSIONS

As briefly as possible, I have attempted to show that the principle of individuation, and the theory of related opposites to which it gives rise, can provide scientific psychology with a discipline of thought covering the universe of discourse, and promote clearly defined hypotheses for experimental investigation. The statistical treatment of quantitative studies also finds its appropriate place and function as a means of determining the degree to which any qualities assessed are in fact genuine dimensions of personality.

Permanent matter and creative mind appear to be two opposite concepts we form of one continuously evolving flow of change of which we are part. It would also seem that the co-ordinates of introverted-extraverted awareness, intellectual organization, affective appreciation and persistent change together provide a sufficient framework of reference for a coherent description of human conduct. That they also provide the minimum number of dimensions required for the comparative study and assessment of personality is a speculative hypothesis, but one which permits of being tested experimentally.

Among the questions I have formulated, one appears to have been overlooked, namely: In any given situation why should a person choose to behave in one way rather than another? This must be the topic for a separate discussion. It may be the exception proving that the four co-ordinates I have delineated provide an adequate basis for a systematic study and description of human conduct.

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USE OF THE EEG IN THE STUDY OF IMAGERY

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An experiment was designed to test the hypothesis that suppression of the amplitude of the alpha rhythm provides a reliable objective index of visual imagery under two conditions of problem solution, namely, solving a 'verbal' problem and solving a 'visual' problem. Sixty-nine subjects were examined in the following way: first, preliminary 'runs' were made with each subject for EEG recording under the conditions (*a*) eyes-shut, relaxed and (*b*) eyes-open, relaxed, yielding base lines for comparison; secondly, EEG recordings were made under the conditions (*a*) concurrently with solution of the 'verbal-reasoning' problem and (*b*) concurrently with solution of the 'visual-reasoning' problem.

It was found that, although the visual content of awareness during the two problem-solution periods as reported by the subjects differed markedly, suppression of alpha rhythms occurred under both conditions, the amplitudes in both cases falling about half-way between the two values recorded in the preliminary 'runs'. There was, however, a greater suppression effect in the case of the 'visual' problem. The results, as a whole, therefore, suggest that the hypothesis is not tenable, since 'imagery' appears to be only one of many factors that may produce suppression effects. This conclusion is supported by a breakdown of the subject sample, according to reports of imagery on the verbal problem, which showed that there was no difference in the basic patterns as between those who visualized the verbal problem and those who did not.

Repeated solution in a check period, immediately following the solution period, made no significant difference to suppression of alpha in the verbal problem, but did so in the visual problem. The suggested interpretation follows Fisher (1916) in regarding this as a possible decrease in image content.

I. INTRODUCTION

Many attempts have been made to discover objective means of assessing imagery, always with discouraging results (cf. Angell, 1910; Fernald, 1912; Woodworth, 1939). None has been able to free itself from an ultimate appeal to introspection ('verbal report') as a validating criterion. This is true of a recent procedure, originally suggested by Golla, Hutton & Grey Walter (1943), and followed up by Short (1953). Still more recently (Short & Grey Walter, 1954), an attempt has been made to use performance measures, though here the precise relationship to imagery seems doubtful. The hypothesis propounded is that a relationship exists between the alpha rhythm of the EEG and the mental process of visualizing. Short's finding purports to demonstrate that the suppression of the alpha rhythm provides an objective index of visual imagery, and it is this claim that the author has set out to examine further. The critical passage in Short's paper is that which reads: 'It was found (*a*) that of the two main imagery types, visualists breathed regularly and showed frequent blocking of alpha rhythms, both of these physiological characteristics being associated with actual visualizing. Verbalists, on the other hand, breathed irregularly with alpha persistences, both characteristics being associated with verbal-motor imagery. As a rule, when verbalists attempted a problem requiring predominantly visual images, then physiological changes occurred characteristic of visualists' (pp. 50–1). It would appear to follow from this statement that the suppression of alpha rhythms provides an objective indicator of the process of visualizing, i.e. the visualizing subject reveals suppressed alpha rhythms whereas the verbalizing subject does not. Doubt has

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already been cast on this claim (Drever, 1955) following a study of the EEG patterns of early blind, late blind and sighted subjects.

It would seem that a reliable objective indicator must satisfy the conditions:

(i) that its operation (variation) should be specific to the mental process it is said to indicate;

(ii) that it should occur under all conditions in which the said process is present, and should not occur in those conditions in which the process is absent; and

(iii) that, failing the above, the particular conditions under which it does operate as an indicator should be precisely specified.

Therefore, if suppression of the alpha rhythm is to be regarded as a valid objective criterion of visual imaging, it should occur whenever the subject is visualizing and only when he is visualizing. Or, if other factors enter as influences they must be specified and amenable to experimental control. Otherwise it will not be possible to examine an EEG record and conclude, from the recorded behaviour of the alpha rhythm, that, at any point, the subject must have been indulging in visual images. Short does not appear to be in opposition to this argument when he observes that verbalists behaved physiologically like visualists on visual imagery-provoking tasks.

But why introduce the notion of imagery types at all? It is at best a confused notion which can throw little light on the problem. Little can be gained from an endeavour to relate a precisely observable and recordable physiological variable with some hypothetical type classification based on 'a tendency to' or 'a habitual mode of' doing something. Surely the important consideration, in the establishment of functional relations, is to ask, regardless of supposed habitual tendencies, what the subject is actually doing in this specific situation. If he is visualizing in this particular case he is a visualizer for the purpose of the investigation. If he is verbalizing he is a verbalizer with respect to the study of the particular situation, and whether, habitually or as a general rule in a wide variety of situations, he is or is not one of these things seems to be irrelevant. The significance of this point is brought out clearly in Short's own findings where, unless the figures are misinterpreted, of a total of 384 responses made by the so-called 'verbalists,' 196, or 51 % are placed in the category of visual imagery. The classification of individuals into imagery types on the basis of the pooled verbal reports from a variety of tasks, including some problems and a number of free imagination situations, and then using this as a dichotomy against which to align a physiological phenomenon associated with a particular task is a dubious procedure.

Many years ago Watt (1906) made the point that the kind of imagery utilized in thinking was a function of the kind of task. This contention of Watt has suggested a method of approach to the present problem which rejects the 'imagery-type' notion in favour of the 'specific-task' criterion. Ideally, one task which necessitated visualizing and another which eliminated it would yield data from which the relation between alpha suppression and visualizing could be tested in a crucial fashion. Such an ideal cannot be attained, yet it is possible, from the multitude of imagery studies, to provide kinds of tasks that are likely to encourage visualizing and others that do not, at least to the same extent.

It was decided, for the purpose of the present experiment, to restrict the number of tasks to two only, the one a 'verbal-reasoning' problem likely to encourage verbal-motor behaviour rather than visual imagery; the other designed to force the subject into visualizing. Verbal reports were called for, not for the purpose of obtaining an over-all imagery-type classification, but simply to establish an 'imagery-present' or 'imagery-absent' response in the particular task, on the subliminal versus supraliminal psychophysical analogy. In the long run, perhaps, there is methodologically little to choose between the immediate report 'Imagery present' or 'Imagery absent' in a problem-solving experiment and the report 'Heavier' or 'Lighter' in a weight-lifting experiment. If the image is subliminal it may be regarded as 'not there' for the subject in the same sense as a subliminal illumination is 'not there', provided that the procedure allows for a report of it if it is there without suggesting that it ought to be. It was further decided that, as the amplitude and the degree-of-suppression of the alpha rhythm varied from subject to subject, crude measurements should be taken from the tracing of an automatic wave analyser, to be treated as ordinary individual-difference phenomena rather than placing the record in a 'responsive' or 'non-responsive' category.

II. THE EXPERIMENTAL PROCEDURE

The present investigation set out to test whether the hypothesis *that the suppression of alpha rhythms is an objective indicator of visualizing* is tenable under the condition of problem solving. The tasks were such as to demand a definite answer to a specific question. It was hoped in the selection of tasks to vindicate Watt's contention that the type of imagery used is a function of the task.

(i) *The subjects*

There were fifty-one subjects. All were male, adult volunteers from University Colleges and senior schools in Oxford. Ages ranged from 17 years 4 months to 31 years 6 months ($\bar{x}=21\cdot7$, $\sigma=3\cdot24$).

(ii) *The tasks*

There were two tasks only, which will be referred to as (*a*) the Verbal Problem (Problem Condition 1) and (*b*) the Visual Problem (Problem Condition 2), without suggesting that the subject is restricted by the nature of the task to single mental processes of verbalizing or visualizing. It was essential that the problems be presented to the subjects in such a way that they could be solved with the eyes closed, so that EEG recordings could be made at the same time. This was achieved by dividing the task into two phases, (*a*) the acquisition of the essential information and (*b*) arriving at an answer to the problem question. The subject was therefore allowed to acquire the information on which the problem was to be set before any recording was made. Then, with the subject's eyes closed, the recording started as the problem question was asked. The information was presented on cards which the subject was permitted to examine until he was able to repeat the contents.

The Verbal Problem was a modified item from a well-known reasoning test, presented as follows:

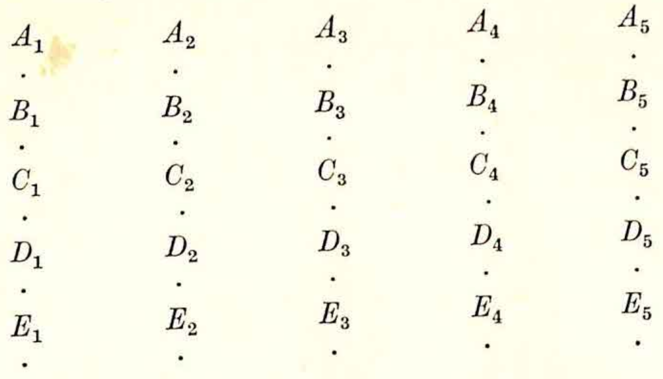
Information: (1) Digby is *five* days *younger* than Browne.
(2) Digby was born on 28 December.
(3) Christmas day is on *Friday*.

Question: 'On what day of the week is Browne's birthday?'

The Visual Problem was a figure-construction task, presented as follows:

Card I. Learn this sequence: B_3, C_2, C_5, E_3, B_3.

Card II. Examine this pattern of dots and notice how they are labelled:



Question: 'Starting from the point B_3, the first point in the sequence you learned, if you draw a straight line to the second point in the sequence, then from there to the third point and so on to the end of the sequence, how many triangles would be formed?'

It was believed that these tasks would be sufficiently unlike in the type of solution processes evoked to throw light on the relation between alpha suppression and visualizing, since, as far as the experimenter could see, the second task was extremely likely to involve visual imaging, whilst the first task was more likely to require verbal-motor processes of counting, reciting, naming and the like. This expectation was confirmed in the verbal reports as will be seen later. The problems were not intended to be difficult ones, the aim being merely to present some different kinds of problem material on which the subject could work mentally for a period of about half a minute or more in order to yield sufficient alpha rhythm data for subsequent measurement.

(iii) *Procedure*

The entire examination was conducted in the EEG department of the Warneford Hospital, Oxford, by permission of the Physician Superintendent. Two persons were present at each examination, namely, the experimenter and the hospital's electroencephalographer. On his arrival at the laboratory, the subject was seated and given a brief informal explanation of the recording procedure with an assurance that he would feel nothing. The apparatus used was the standard eight-channel Ediswan Mark II EEG and an Ediswan eight-channel automatic wave analyser. While the subject was settling down, a brief explanation of the apparatus and procedure was given, together with the information that the experimenter was interested in brain changes occurring in problem solution. Detailed instructions for the tasks, the necessity for relaxation, and the requirement to keep the eyes closed, except when otherwise ordered, were emphasized at this stage. The arrangements for presentation of the information cards and problem questioning were then explained.

As soon as the subject was clear on the details of procedure, the electrodes were fitted, one pair on the left side and one on the right in the parieto-occipital area.

The subject then lay down on his back on the couch, his head supported by pillows and, while the electrodes were connected to the input stand, he was informed of the necessity for lying very still throughout the procedure, and again reminded of the 'eyes-closed' requirement and the desirability of relaxation.

The next step was to take a trial run which consisted of a 30 sec. recording, the subject lying relaxed with eyes closed, then 10 sec. with eyes open, and a further 20 sec. with eyes closed. The purpose of the trial run was to establish two basic measures, a maximal and a minimal for each subject, with which the problem-solving records could be compared. These two measures will be referred to as Trial Run A and Trial Run B respectively. At the end of the trial run the subject was presented with the information card for the first problem (verbal).* When he had indicated his readiness to proceed by reciting the items of information, he was told to relax and close his eyes. Then, as the experimenter asked the question 'On what day of the week was Browne's birthday?' the EEG record was commenced. When the subject gave his answer, the experimenter said, 'Keep your eyes closed, check over it and give me your answer.' After the second answer, the instruction was given 'Alright. Now relax and forget about the problem.' The recording was continued for a further ½ min, then the subject was allowed to open his eyes. At this stage he was asked to give a report on what he had done, how he had arrived at the answer, and what went on in his mind during the work period. In the early preliminary trials on the first problem it became evident that subject after subject made no spontaneous reference to visual imagery, so it was decided to ask a question that would provide the opportunity to report visual imagery if it occurred. The question was 'Did you have any special way of representing the information to yourself to help you remember it? Did you introduce any aids to enable you to organize the material?' The procedure for the second (visual) problem was the same as the above except that two information cards were presented, one containing the letter-number combinations to be learned, the other containing the square arrangement of labelled dots. In the case of the verbal report following the solution of this problem it was unnecessary to ask the supplementary question because in every case the role of visual imagery was made quite clear in the spontaneous report.

III. RESULTS

The ink path traced by the recording pen of the automatic analyser was taken as the source of data for the compilation of results. Taking every subject's record separately, each frequency within the accepted alpha band (8–13 c./s.) was measured for height in millimetres within each 10 sec. epoch, then averaged over the number of epochs occupied on each of the experimental 'treatments'. This procedure yielded for individual subjects four sets of values or 'scores'.

(i) Average height in millimetres of each frequency in the alpha band for recordings under the condition 'eyes-closed, relaxed, no problem'. This will be referred to as Trial Run A (Condition A).

(ii) Average height in millimetres of each frequency in the alpha band for recordings under the condition 'eyes-open, relaxed, no problem'. This will be referred to as Trial Run B (Condition B).

(iii) Average height in millimetres of each frequency in the alpha band for recordings under the condition 'eyes-shut, solving the verbal problem'. This will be referred to as Problem Condition 1.

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* Consideration was given to the desirability of counterbalancing the order of presentation of the problems. This course was rejected for the following reason. It was thought desirable to evoke a 'natural' approach to each problem, as each was designed to bring to the fore different processes of solution. There seemed, rightly or wrongly, more likelihood of 'suggestion' spreading from the 'visual' to the 'verbal' task than vice versa, because the solution of the 'visual' problem by other-than-visual means seemed most improbable, whereas the solution of the 'verbal' problem with other-than-verbal means seemed more likely, and avoidance of a 'spread-of-method' from the visual task was sought.

(iv) Average height in millimetres of each frequency in the alpha band for recordings under the condition 'eyes-shut, solving the visual problem.' This will be referred to as Problem Condition 2.

(i) *Analysis of the results as a whole*

It was decided, before proceeding to a statistical analysis of the results, to graph the average 'scores' for the fifty-one subjects for all frequencies and all four conditions, to see if there was any significant pattern which might show the result as a whole.

The outcome of this decision may be seen in the numerical form in Table 1, and graphically in Fig. 2.

Table 1. *Average height in millimetres of analyser tracing for each frequency* (8–13 c./s.) *and each experimental condition* ($n=51$)

	Alpha band (c./s.)					
Condition	8	9	10	11	12	13
Trial Run A	13·1	25·7	30·7	20·7	14·1	6·7
Problem Condition 1	9·3	17·7	21·1	15·8	10·9	5·8
Problem Condition 2	8·5	17·0	20·6	14·6	9·9	5·1
Trial Run B	7·1	9·0	10·0	7·6	7·0	4·4

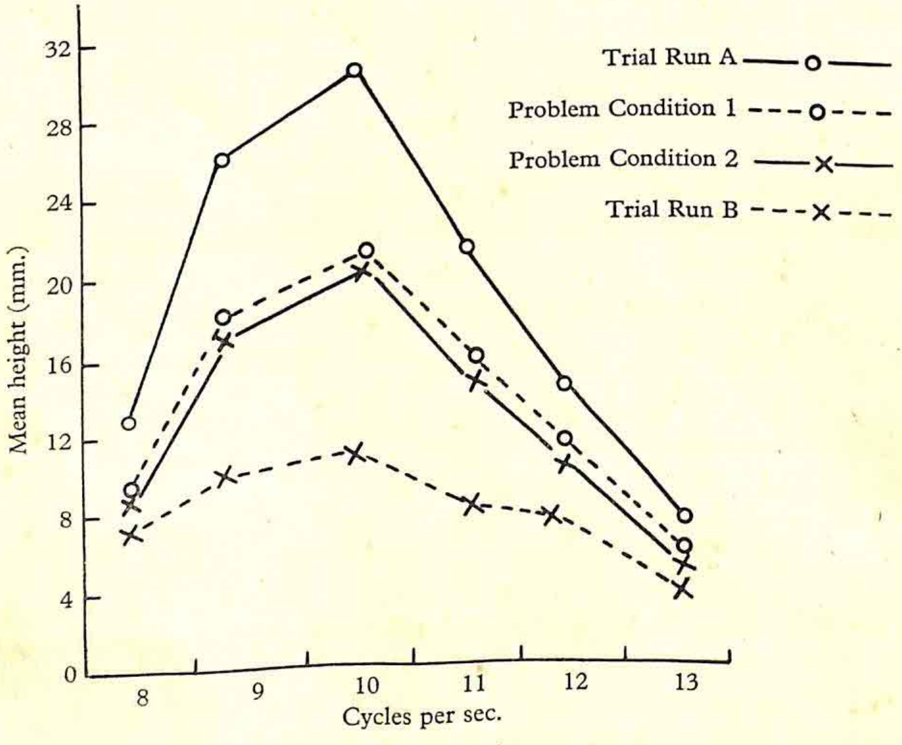

Fig. 1. Graph of average height in millimetres of analyser tracing for each frequency (8–13 c./s.) and each experimental condition. $n=51$.

A regular trend in the results for the various 'treatments' is immediately apparent. Comparing Trial Run A with Trial Run B averages, it will be observed that there is a very marked decrease in alpha amplitude at all frequencies as between the 'eyes-closed, relaxed' condition and the 'eyes-open, relaxed' condition. It will also be observed that there is a marked decrease for both problem conditions, these following a very similar course about midway between Trial Run A and Trial Run B. The graph shows that both problem conditions influence the behaviour of the alpha rhythm in the same way.

Disregarding for the time being the subject's verbal reports, it remains to test the significance of the differences of means for the various conditions as revealed in the table and graph. This could be done for each of the frequencies separately, but it was decided for reasons of sheer computational labour-saving not to calculate significance values for each of the six frequencies in the band, but to combine the values for the three dominant frequencies in each subject's records to give a single 'score' for each individual on each of the four 'treatments'. The aim here was to bring out as clearly as possible the overall picture of alpha suppression under the different conditions. It was found in all cases that, though the particular frequency that was dominant was not regularly 10 c/.s., the dominant frequency (whatever it was) and the two frequencies adjacent to it accounted for practically all the variation in amplitude. Though there may be some doubt as to the absolute validity of the summing procedure, it seemed reasonable enough as an approximation to measurement of the total effect.

The summation procedure yielded for each subject four 'scores' corresponding to the four conditions:

(1) Average of the three selected alpha amplitude values in Trial Run A.
(2) Average of the three selected alpha amplitude values in Trial Run B.
(3) Average of the three selected alpha amplitude values under Problem Condition 1.
(4) Average of the three selected alpha amplitude values under Problem Condition 2.

Taking the distribution of these 'scores' for the fifty-one subjects under the four conditions and comparing them two at a time, it was possible to calculate values of '*t*' to indicate the probability that the observed divergences arose by chance. Table 2 lists the mean values for the conditions and the raw differences upon which *t* values are established.

Table 2. *Mean values and raw differences between means for the distributions of 'scores' obtained under four experimental conditions taken two at a time.* $n = 51$

Comparison	Mean_1	Mean_2	Difference
Trial Run A *v.* Problem Condition 1	27·637	19·902	7·735
Trial Run A *v.* Problem Condition 2	27·637	17·696	9·941
Trial Run A *v.* Trial Run B	27·637	9·196	18·441
Problem Condition 1 *v.* Problem Condition 2	19·902	17·696	2·206
Problem Condition 1 *v.* Trial Run B	19·902	9·196	10·706
Problem Condition 2 *v.* Trial Run B	17·696	9·196	8·500

Testing the significance of these obtained differences by the method advocated by Lindquist (1940) for the calculation of '*t*' where the measures are correlated, the values shown in Table 3 are obtained.

An examination of the tables reveals that all the obtained differences are statistically significant at a very high level. The position may be summarized as follows:

(1) There is a very significant suppression effect on alpha amplitudes in each of the two problem conditions as compared with the 'eyes-closed, relaxed' condition of Trial Run A.

(2) Both problem conditions yield significantly higher amplitude values than the 'eyes-open, relaxed' condition of Trial Run B.

(3) The difference in amount of suppression effect between Problem Condition 1 (verbal) and Problem Condition 2 (visual), though the absolute value of 't' is much smaller than in other comparisons, is still highly significant, the suppression being greater in the case of the visual problem.

Table 3. *Values of 't' and 'p' for differences of mean 'scores' obtained under the four experimental conditions taken two at a time.* $n = 51$

	Trial Run A	Problem Condition 1	Problem Condition 2
Problem Condition 1	$t = 7{\cdot}900$ $p < 0{\cdot}001$	— —	— —
Problem Condition 2	$t = 8{\cdot}844$ $p < 0{\cdot}001$	$t = 3{\cdot}745$ $p < 0{\cdot}001$	— —
Trial Run B	$t = 9{\cdot}196$ $p < 0{\cdot}001$	$t = 5{\cdot}918$ $p < 0{\cdot}001$	$t = 5{\cdot}212$ $p < 0{\cdot}001$

The statistical outcomes are not surprising when the individual subjects' 'score' patterns are examined. These show a remarkable regularity of trend with very few reversals, even as between Problem Condition 1 and Problem Condition 2 where the differences are small. The tendency throughout is for the Trial Run A value to be relatively large, the Problem Condition 1 value to be considerably smaller, the Problem Condition 2 value to be slightly smaller still, and the Trial Run B value to be again considerably smaller. The overall effect for the fifty-one subjects may be seen in the graph of Fig. 2.

Similar graphs had been drawn when $n = 20$, $n = 25$, $n = 30$ and $n = 41$, and at no stage was there any suggestion of a change in this basic pattern, thus yielding further evidence of the regularity of the trend.

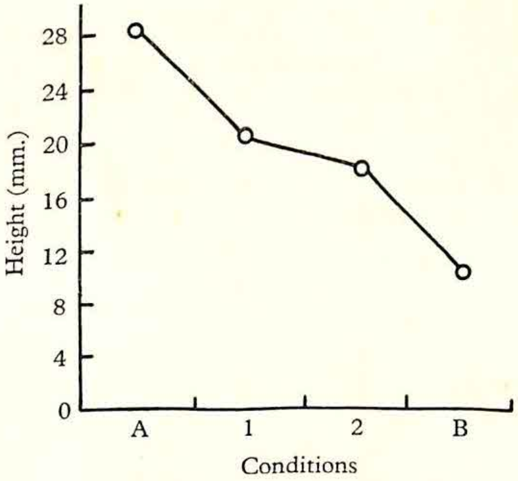

Fig. 2. Graph of the average alpha amplitudes (combining three dominant frequencies) under the four experimental conditions. $n = 51$.

(ii) *Analysis for two subsamples*

So far in the presentation of results no account has been taken of the question whether the problems were in fact dependent upon different mental processes. No reference has been made to the subjects' reports recorded verbatim at the conclusion of each problem solution. The physiological changes for the two problems have been compared just as they stand, and not without some justification as a first approximation. For, when the *spontaneous* reports for two problems are compared, a striking difference is noticeable. In the case of Problem Condition 1, there was no spontaneous report that included any reference to visual imagery. That is, without supplementary questioning, there was no evidence that visual imagery played any part in arriving at a solution to the problem. In the case of Problem Condition 2, on the other hand, every subject spontaneously referred to the part played by visual imagery in his approach to the problem. The examples quoted hereunder are typical of the kinds of verbal reports elicited by the two problems:

Subject 2

(*a*) *Verbal problem.* 'I took Christmas Day as the starting point, the 25th. The 28th is three days later and five days before that is the 23rd, i.e. two days before Christmas which gives Wednesday. Then I got confused about whether I should count back five days, or five days inclusive, hence I got two answers, Wednesday then Thursday.'

(*b*) *Visual problem.* 'I knew that the first line was going to be a diagonal, then I sort of pictured the square and drew a kind of imaginary line from B_3 to C_2, then across to C_5, then to E_3 and back to B_3. I pictured the triangles as a rather flat one on top and a larger one below.'

Subject 23

(*a*) *Verbal problem.* 'For a moment I went rather blank, then I thought Digby was born on the 28th, therefore Browne was born on the 23rd. Christmas Day was two days later and on Friday, therefore Browne was born on Wednesday.'

(*b*) *Visual problem.* 'Here I visualized the figure, not as a whole, but first from B_3 to C_2 which I could not quite see at first, then the straight line to C_5, then E_3 back down, and to B_3, a straight line up again, giving two triangles. When I checked over it I saw the lines as they occurred, making a sort of figure 8. It was fairly clear—top triangle small, bottom one bigger.'

Subject 35

(*a*) *Verbal problem.* 'I was a bit "flummoxed" at first. Then, since Christmas Day is on Friday, Digby's birthday, the 28th, is on Monday, and as Browne was born five days before that, it must be Wednesday. Then I realized that I could have done it a different way—as Christmas Day is three days before Digby's birthday it must be two days after Browne's which must be Wednesday.'

(*b*) *Visual problem.* 'Well, I got the position of B_3 and the position of C_2, then I saw that it went along from there in a straight line, then it went down, then straight up again. When I saw that the last line went straight up, I saw that it formed two triangles. I sort of saw the thing grow as I traced it out, and then it fell into a pattern.'

There is little need to add further examples. All follow similar lines and the difference between the two types of reports is obvious. At the same time it was deemed necessary to go beyond the spontaneous report in the case of Problem Condition 1 and give subjects the opportunity to report visual images if they were present. For this purpose the supplementary questioning already referred to was included after each spontaneous report and, in ten cases out of the fifty-one, the questioning elicited a reference to the presence of visual images. Some representative examples of the two types of answer given to this question are quoted below.

Verbal problem—images reported

Subject 10. 'I just remembered them successively as I wanted them and there was some visualization, e.g. when I was working back from Friday. I seemed to see the word "Thursday". I also saw "28th", not as on the card, but detached.'

Subject 15. 'I simply calculated, working from my birthday, but I pictured two men, one Digby and one Browne.'

Subject 16. 'I tried to get a pictorial method of arranging the information, remembering it as it was printed, but it seemed to get away from me.'

Subject 25. 'I tried to visualize the material as entries in a diary and could see the facts entered—a strong visual impression of the facts.'

Verbal problem—images not reported

Subject 11. 'Just seemed to keep the information in mind—condensed to the essential things.'

Subject 19. 'I learned it by heart, remembered the "word formula" then let it go, bringing it back as I required it.'

Subject 35. 'I just seemed to put the facts in the back of my mind and called on them as required.'

Subject 40. 'No special form of representing the facts. I just kept the 28th in mind, the facts just seemed to stay there, and I counted silently on my fingers actually.'

There is, then, a division within the subject sample with respect to the verbal reports on Problem 1. Ten subjects reported the presence of visual images and forty-one did not. Whilst ten is scarcely a large enough number on which to base a reliable conclusion, separate analyses of the two subsamples were made to see if there was any marked difference in the general pattern of results as between subsamples. The patterns are represented graphically in Figs. 3 and 4.

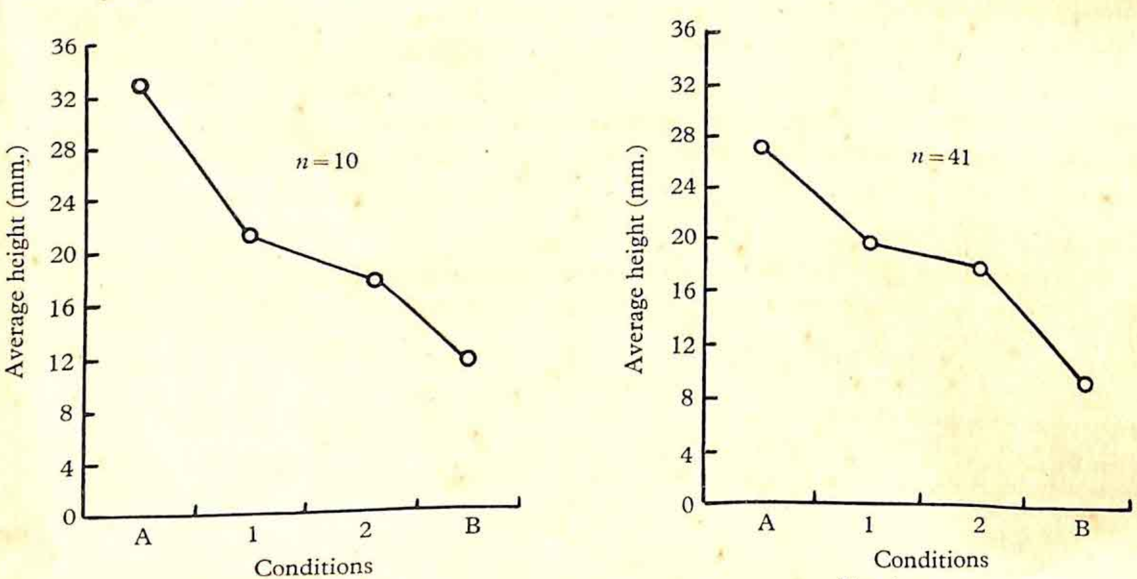

Fig. 3.

Fig. 4.

Figs. 3 and 4. Graphs of the average 'scores' of two subsamples, divided in accordance with reports *v.* non-reports of visual imagery in Problem Condition 1.

The two graphs are clearly not identical but show the same general trends, namely, a substantial drop from Trial Run A to Problem Condition 1, a further small drop to Problem Condition 2, and another substantial drop to Trial Run B. The alpha rhythms of both groups of subjects behave in much the same way except that, in general, the non-visualizing group ($n=41$) tends to yield lower average magnitude values all round. This may or may not be significant and there is no reliable way of deciding the issue on the available data. If, however, Trial Run A is taken as a sort of 'baseline', and the amount of suppression under the various conditions is expressed as a percentage loss on this value, it may be possible, again allowing for the probable sampling errors, to obtain information on differences between the two subsamples of subjects.

Table 4 summarizes the statistical situation, showing the percentage losses of amplitude for each subsample under each condition.

Table 4. *Average amplitude values for the two subsamples under the four experimental conditions, showing percentage losses when Trial Run A is taken as a 'baseline'*

	Subsample $n=10$		Subsample $n=41$	
Condition	Average amplitude	Percentage loss	Average amplitude	Percentage loss
Trial Run A	33·20	—	26·52	—
Problem Condition 1	21·15	36	19·60	26
Problem Condition 2	17·35	48	18·00	32
Trial Run B	11·45	66	8·65	67

Whereas the percentage loss from Trial Run A to Trial Run B is similar for the two subsamples, the reductions in the problem situations appear to be quite different. There is a greater tendency for the subsample reporting visual imagery under Problem Condition 1 to show suppression of the alpha rhythm in both problem conditions than for the larger subsample reporting no such imagery. As an offset to this finding, which would seem to support the imagery suppression hypothesis, there is the further result that the difference between Problem Condition 1 and Problem Condition 2 appears to be greater for the subsample reporting visual imagery in both problem conditions than it is for the subsample reporting no visual imagery in the first problem condition and imagery in the second, where a larger difference might be expected from the hypothesis.

Table 5. *Values of 't' for the two subsamples taken separately, showing significance of differences between mean alpha amplitudes in the four experimental conditions*

Comparison	Mean$_1$	Mean$_2$	Diff.	t	p
	Subsample $n=10$				
Trial Run A *v.* Problem Condition 1	33·20	21·15	12·05	4·10	<0·01
Trial Run A *v.* Problem Condition 2	33·20	17·35	15·85	6·15	<0·001
Trial Run A *v.* Trial Run B	33·20	11·45	21·75	6·77	<0·001
Problem Condition 1 *v.* Problem Condition 2	21·15	17·35	3·75	4·88	<0·001
Problem Condition 1 *v.* Trial Run B	21·15	11·45	9·70	4·63	*c.* 0·001
Problem Condition 2 *v.* Trial Run B.	17·35	11·45	5·90	2·84	<0·02
	Subsample $n=41$				
Trial Run A *v.* Problem Condition 1	26·52	19·60	6·92	7·26	<0·001
Trial Run A *v.* Problem Condition 2	26·52	18·00	8·52	7·89	<0·001
Trial Run A *v.* Trial Run B	26·52	8·65	17·87	7·72	<0·001
Problem Condition 1 *v.* Problem Condition 2	19·60	18·00	1·50	3·09	<0·01
Problem Condition 1 *v.* Trial Run B	19·60	8·65	10·95	5·00	<0·001
Problem Condition 2 *v.* Trial Run B	18·00	8·65	9·35	4·84	<0·001

The most cogent evidence to be ranged against the strict relation between visual imagery and suppression of the alpha rhythm comes from the tests of significance between the means for the various experimental conditions when applied to the two subsamples separately. An inspection of the values of 't' in Table 5 makes it clear that significant loss of alpha amplitude cannot be taken as an objective indicator of supraliminal visual imagery.

Since the suppression effect, despite variations, is significant throughout both subsamples, there is little to support an argument for alpha-amplitude loss as an objective measure of visual imagery. Both subsamples behave in very much the same fashion throughout with respect to the significance of alpha-amplitude losses in the experimental conditions.

(iii) *Comparison of values for solution period and check period*

The final check on the hypothesis rests on rather tenuous grounds and cannot, perhaps, be taken too seriously. It has been claimed by Sara Fisher (1916) and others that in thinking situations a task requiring effortful visualizing in the process of arriving at an answer often loses a good deal of its image content upon repeated solutions, as it comes to take on a more verbal formulation. Now, if one task does not, on the whole, require or involve visual imagery in its solution, it might be predicted that repeated solution, as was required in the check period of this experiment, would have no appreciable effect. If the second task does require effortful visual imagery in its original solution, it might be expected that the image content would be lessened in a subsequent solution. Therefore, if the verbal task should show no significant differences between alpha amplitudes for successive solutions whereas the visual task showed a significant gain from initial solution to check solution, the different behaviours might possibly be accounted for in terms of the suppressing effect of visual imagery, suggesting that visualizing contributes to the overall suppression effect and may in fact account for the differences between Problem Condition 1 and Problem Condition 2, though not accounting for the great bulk of the suppression effect. That something like this may have happened in the present experiment may be seen from a comparison between Problem Condition 1 and Problem Condition 2 with respect to changes occurring from solution to check period, shown in Table 6.

Table 6. *Comparison of amplitude values in solution period and check period for the two problem conditions*

	Mean value, solution period	Mean value, check period	Diff.	t	d.f.	p
Problem Condition 1	19·42	20·45	1·03	1·89	50	<0·05
Problem Condition 2	16·74	19·54	2·80	4·80	46*	<0·001

* Four subjects did not repeat the solution process.

It is evident that the gain in amplitude during the check period for Problem Condition 1 is very slight and does not reach the 5 % level of significance, whereas the rise from solution to check period in the case of Problem Condition 2 is highly significant. It seems possible, therefore, that active visualizing is A factor in the suppression of alpha rhythms but not by any means THE factor. As a matter of observation verbal-motor behaviour is common to both tasks, and if the check period for Problem Condition 2 is compared with the solution plus check values for Problem Condition 1, the differences again are not significant, yet each is significantly different from Trial Run A. Table 7 shows the comparison between Problem Condition 1 (solution and check) values and Problem Condition 2 (check only) values.

All that can be said from the separate treatment of the initial solution period averages and the check period averages is that, whilst both show the same sort of suppression effects when compared with Trial Run A as a baseline, there is a suggestion that visualizing may account for the difference between Problem Condition 1 and Problem Condition 2. This, of course, is highly speculative and requires further investigation.

Table 7. *Comparison of average values for Problem* 1 (*total solution plus check*) *and Problem* 2 (*check period only*)

Problem Condition 1 (solution plus check)		Problem Condition 2 (check only)	Difference
20·09	$t = 0·84$ d.f. $= 46$ $p > 0·10$	19·57	0·52

IV. Discussion

All the evidence so far educed from the statistical data and verbal reports indicates that although image-provoking problem situations reduce the amplitude of the alpha rhythm, the converse proposition that amplitude reduction is a measure of visual imaging is not confirmed. Amplitude reduction is associated with visual imagery and with a number of other conditions as well; among those listed, for example, by Jasper & Cruickshank (1937) being problem solving generally, after-sensations of colour, trying to see in the dark, autonomic 'tension', sound stimulation, and tactual and pain stimulation. Stimulus input representing a 'noticeable' change, whether from external or internal sources, appears to be the general condition under which the amplitude of alpha rhythms changes. Grey Walter's (1950) speculation of the possibility that 'the spontaneous rhythms do in fact play the leading role in interpreting sense data to consciousness' has more widespread theoretical implications for psychology than the narrowing of the field to the 'measurement' of visual imagery supported in Short's paper. If the suppression of alpha amplitude were confined to visual perception and visual imagination, the discovery and confirmation of this phenomenon would have considerable, though narrow, psychological significance, since it would go a long way towards settling the problem of the relation between imagery and perception, indicating the similarity of the brain mechanisms involved in the two processes. The evidence from the present experiment and others cited in the literature points away from such a specific functional relation and suggests a mechanism of far wider significance from a theoretical viewpoint.

Returning, then, to the criteria of an objective index proposed at the beginning of this paper, it is clear that suppression of the amplitude of the alpha rhythm in EEG recordings satisfies neither the requirements of specificity, nor of agreement-and-difference demanded there. In other words, it is not possible to inspect an EEG record in a problem-solving situation and, from observation of the behaviour of the alpha rhythm, to state that the subject under examination is or is not indulging in visual imagery. Further, at the present stage of inquiry it is not possible to state the precise conditions under which the phenomenon in question could be regarded as an objective indicator of imagery. The 'other-things-being-equal' argument in this case introduces a vagueness that makes any sort of precision of statement difficult, since it cannot be known with any certainty what the variables are that have to be 'equal'.

Certainly visual perception has a marked suppression effect. Visual imaging in a problem-solving situation has a similar though less marked effect, and it is not unreasonable to suppose that the brain events in imaging are similar to those involved in perceiving, probably activating and being acted upon by the same 'trace systems', 'neural schemata', 'reverberatory circuits' or what you will. But the phenomenon is by no means restricted to brain events associated with visual processes. It seems rather to be involved in more general situations of an 'attention-getting' or 'concentration' kind. Situations that disturb equilibrium and demand adjustment, whether the disturbance arises from external or internal sources, tend to produce the effect. There may be an important mechanism of adaptation to sensory (including imaginal and autonomic) disturbance involved, a notion that gains some support not only from the evidence already cited, but also from the 'adaptation' and 'switch-off' effects observed by Jasper & Cruickshank (1937).

The possibilities of teasing out the facts of the matter are fascinating. It could be that the suppression of alpha is another sign of tension like the G.S.R. and will resist attempts to pin it down to more definite and theoretically more interesting psychological phenomena. It remains for further researches to demonstrate whether psychologically meaningful relationships are there to be discovered.

The present study is a narrow one directed to the tenability of a particular hypothesis under specified conditions. The evidence supplied by it supports a negative finding.

The author wishes to acknowledge his debt to the British Council for the Scholarship which made the investigation possible. Thanks are also due to Prof. George Humphrey, Oxford, who supervised the project; to Dr R. G. McInnes, Physician Superintendent at the Warneford Hospital, Oxford, who made the hospital's facilities available for the research; and to Mr Douglas Lee, electroencephalographer at the Warneford, whose co-operation was a major contribution to the conduct of the investigation.

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A THEORY OF INSIGHT, REASONING AND LATENT LEARNING

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The types of performance to be explained, i.e. reasoning and latent learning, are briefly delimited. The type of structure which would exhibit such behaviour is outlined. The assumption of such a structure is then shown to explain the relevant experimental data. Some predictions are also made.

INTRODUCTION

Animals under some conditions can draw upon past experiences acquired on diverse occasions to solve a problem without trial and error. When the problem is not a novel one, we say that the animal has learnt and we point to a process of trial and error which took place when the problem was new. These situations have been dealt with in a previous paper (Deutsch, 1953). There are, however, cases in which the animal solves a novel problem without trial and error, though it draws upon information about the situation acquired on previous occasions, perhaps under conditions of different motivation and reward. These cases are called instances of latent learning and reasoning. We talk of reasoning when the animal has to combine two sets of information in some way in order to find a solution. All the information which the animal must utilize in order to solve the problem is never presented together. The animal is then set a goal which it can only reach without error by 'putting together the various pieces of knowledge'. In latent learning experiments the situation with which the animal is confronted is similar. The difference lies in the fact that all the evidence which the animal will need to use in order to reach the goal which will be set it, is presented together, sometimes in the order in which it will occur in the test and sometimes in an order determined solely by the random explorations of the animal in the maze and the random selection of points of entry and exit by the experimenter.

The word 'insight' has been variously used. It has been spoken of in connexion with such things as taking short cuts. It has also been used where an animal solves a problem without trial and error, when it has not been presented with the relevant information previously by the experimenter as in the latent learning or reasoning situation. Because of this experimental omission it has sometimes been supposed that the animal has in fact solved problems without ever having been presented with the relevant information. If we make this assumption it must be that either the animal has been so constructed that the information which it needs is already supplied by hereditary factors, in which case the label 'instinct' is appropriate, or that the solution is purely accidental. It seems, however, more plausible to believe that in these cases of 'insight' the information though not presented by the experimenter has been previously acquired piecemeal on diverse occasions (cf. Birch, 1945). If this argument is accepted, then the problem of 'insight' reduces to the same as the problem of latent learning and reasoning, defined above.

THE HYPOTHESIS

A theory of learning and performance under goal-seeking conditions has already been put forward in a previous publication (Deutsch, 1953).

As it is proposed to use essentially the same system with one or two additions to account for the types of ability described above, it is necessary to give a brief résumé of the theory.

The system is made up of five elements. These elements are fully defined by the relations between themselves and other elements. Only some of them can be given a physical identification at present. These elements are called: (*a*) analyser (to be identified with a receptoral system), (*b*) a link, (*c*) a motor (or effector organization), (*d*) an environment, (*e*) a feature of the internal medium (to be identified with a hormone, etc.). These elements can all be considered as the same, differing only in the way that they are connected to the others.

The relations obtained between these five elements can be reduced to three. These are called: (1) activating, (2) switching off, (3) causing to vary.

The following are the rules of the system:

(1) Links are arranged in single rows. That is, each link has only two neighbours and the first and last only one.

(2) Some of the first links in these rows are set into activity by a feature of the internal medium in proportion with the magnitude of this feature (such as dehydration).

(3) The first link excites its neighbour (since any link which is excited, excites the next link) and so on until the whole row is excited.

(4) Each link is connected to a portion of the motor system which it excites when it is itself activated.

(5) When the row of links is active only the last link to be excited will excite its portion of the motor system fully.

(6) The excitation of the motor system causes the features in the environment to vary.

(7) A particular feature in the environment activates a particular analyser.

(8) An analyser decreases the excitation transmitted by any link to which it is attached according to the amount of firing from the analyser. This link will take some time before it resumes full irritability and so before it transmits excitation to links further in the row.

(9) Each link when it is active and exciting the motor system will persist in exciting the same part of the motor system whilst the input from the analyser attached to it increases. When there is a decrease, its motor output will vary. The link will remain connected, when it is switched off, to the particular part of the motor system which it was exciting just before this event, so that when the link is again switched on this output will be present.

These last two propositions represent complications of the original hypothesis, now perhaps warranted by experimental work conducted by the author into the relationship of sensory input to motor output. It is assumed that each analyser, such as that dealing with the reception of a particular pitch, fires to an increasing extent, the closer the correspondence to it of an incoming message. This rate of increase is picked up by a differentiating device. If the correspondence increases, the same action is further continued, if it decreases the course of action is altered. Thus when we endeavour to maintain a certain pitch in singing, we correct when we hear that we are straying away from the set note and reverse our course of action when this last correction has begun to lead us into the opposite error and we perceive a decreasing correspondence. One of the predictions from this view is that a reversal of the sign of the error where a conflict of cues can be excluded should not lead to an impairment of the ability to reach and maintain a note (to use a previous example).

To give a concrete example, the following is assumed to occur when a thirsty animal is running a maze for a reward of water. The 'landmarks' in the maze are taken to be 'represented' by analysers arranged in the order in which the animal has found them on previous runs. The 'cues' or 'landmarks' nearest the goal are 'represented' by analysers connected to the link next in order to that excited by dehydration. Those cues at the entrance of the maze are farthest away on the row of links from this first link. When this first link is excited by dehydration, the whole row of links will be excited link by link. The last link will in all probability be switched off without any activity on the part of the animal as the experimenter sets it at the entrance of the maze. For at this juncture it will see from a distance some other cue to which it will attempt to approximate itself. During this activity further cues will come into view and as this happens the animal will locomote towards these. Any cue nearer the final goal will be selected rather than one farther away—for any analyser which is set off by a cue nearer the goal will be attached nearer the first link from which the excitation in the row is originally derived. When this analyser is set off, the link to which it is attached will cease transmitting excitation to its neighbour, and so cut it off from all the links farther away from the first link than itself. In this way a clearly visible short cut will be taken as soon as it is introduced and blind alleys will be eliminated.

The assumption is made that there exist in the nervous system of an animal prior to learning many sets of units (called links). These links are joined together in rows such that when the first link is excited it excites the second, which in turn transmits this excitation to the third, and so on.

Further, the various 'first links' are each excited by different humoral factors. It is this excitation which is passed from link to link. Each link is connected to the motor system, but will excite it when it is itself passing no excitation to the next link.

This will occur in the following way. Different analysers, or receptoral organizations are attached to each of the excited links. When these discharge they cause the link to which they are attached to switch the excitation which it is transmitting to its own control of the motor system.

These analysers are attached to the first few links innately. To the others they are connected whenever an analyser already connected to a link discharges.

(10) When an analyser already attached to a link in the row is excited the analysers which have been fired the moment before will be attached to the empty links at the end of the row. These fresh analysers which fired off nearest in time to the analysers already attached, will be fastened to the empty links nearest in order (next to) the links already occupied by analysers. Thus, temporal proximity of firing of analysers will lead to ordinal proximity of attachment on the row of links.

This attachment will not take place when the order of stimulation is already recorded in the system. Further, if an analyser attached to a link in the middle of a recorded sequence is touched off, it will be connected to an empty row of links together with the analysers which fired off prior to it (see further rule 14).

Thus the animal will retain a record of the order in which cues occurred in relation to each other and the goal. The factor leading to reinforcement, primary and secondary, is the firing off of an analyser already attached to a row of links. The theory is thus able to reconcile the apparently contradictory results of Kohn (1951) and Miller & Kessen (1952) on one side, and Sheffield, Wulff & Backer (1951) on the other (Deutsch, 1953).

However, on future occasions the firing of one analyser may not be succeeded by the firing of the next, because the environment alters.

(11) The 'threshold' of excitation of the link x, the analyser of which has fired, will rise if it is not switched off, for instance if the analyser attached to the succeeding link does not fire. This threshold will continue to rise while this switching off is delayed.

Thus when the first link in the row is again excited on a subsequent occasion the amount of excitation transmitted to the link x and from it to its neighbour and so to the rest of the row will be diminished.

(12) This 'threshold' of excitation of a link will be lowered each time a link is switched off. The lowering of the threshold will be proportionate to the intensity of excitation present in the link at the time. One of the consequences is that the more often cues arrive in the recorded sequence the more excitation will be passed through the row of links where it is again excited.

(13) If there are two or more rows of links excited, the links at the end of the rows may be setting off movements that are incompatible. In this case the link with the highest rate of excitation will determine the movements.

Hence, as a deduction from rules 12 and 13, when two links are setting off competing actions the one which has fewer links between itself and the first link will obtain precedence, other things being equal.

There will frequently arise occasions on which one analyser will be found to be fastened to two or more different rows of links. A cue will have been noticed which is common to two or more situations at once.

(14) When an analyser is connected to two or more links at once, a small fixed proportion of excitation will be passed through it from one link that may be excited to the others to which it is fixed. (These will transmit this excitation to their neighbours according to rule 3.) These links may either be constituents of the same row or other rows. The greater the number of analysers which two links or rows have attached to them in common, the greater the transfer of excitation from one to the other.

(For the sake of graphic convenience, when there are many analysers common to two links, they will be represented just as a single analyser. This analyser, according to the number it actually represents, will be spoken of as transmitting more or less excitation according to a rough criterion of similarity.) A machine embodying the main rules of the theory has been constructed (Deutsch, 1954).

EXPLANATIONS

Having set up this comparatively economical system we are now in a position to examine how it would explain some of the evidence. Let us begin by taking an experiment originally performed by Tolman & Honzik (1930), and more recently repeated by Caldwell & Jones (1954), in an enclosed maze. We have Osgood's word (1953) for it that it is a case of reasoning to which 'a Hull type analysis would not be applicable'. (This was, it is true, when it seemed that the result could not be repeated.) In the experiment of Caldwell & Jones (1954) the rat was taught three paths to the same goal, a straight alley and two other paths. One of these was much longer than the other and joined the straight alley nearer the goal than the other detour path. After training, the straight alley was blocked at a well-demarcated point before the longer path joined the straight alley but after the shorter detour had already met it. The rats after this experience of the block mostly chose the longest path, the only one which the block did not affect.

Assume that three rows of links were connected to various analysers during the process of learning the three alternative pathways. According to the hypothesis put forward, the analysers attached to a row of links, and their order, correspond to the cues picked up by the rat, and their order, in each alley. However, as reduplication of an order already laid down does not occur, those parts of each path which overlap are all represented on the same row of links. Those parts of the paths which do not overlap are arranged on separate rows (rule 10). One of these three parts will be represented on the same row as the common part of all the paths, depending on which path was learnt first. The other two parts will be organized on separate rows and connected to the row representing the path learnt first via common analysers. ('If an analyser attached to a link in the middle of a recorded sequence is touched off, it will be connected to an empty row of links together with the analysers which fired off prior to it.') When the animal finds that the straight alley to the goal is blocked, then according to rule 11 'the "threshold" of excitation of the link x, the analyser of which has fired, will rise if the analyser attached to the succeeding link does not fire and will continue to rise the longer this is delayed' (Fig. 1). Now both the straight alley and the shorter detour have that portion of the runway in common in which the block is inserted. This means that both paths are represented by the same analysers attached on the same links. Now when this sequence does not recur, there will be a decrease in the excitation which is transmitted past the point where the break in sequence occurred. Consequently, when the animal is again placed at the choice point, it will tend to go via the path which is 'represented' by that row of links in which there is no impediment to the transmission of excitation between the link 'representing' the choice point and that 'representing' the goal. For, according to rule 13, 'if there are two or more rows of links excited, the link with the highest rate of excitation will act on the motor system'. Hence the rat will choose the longest detour.

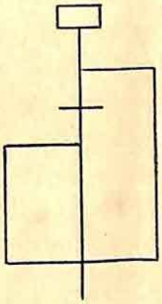

Fig. 1.

Hull (1935) has, however, attempted to explain some of Maier's (1929) experimental data on reasoning. He uses an imaginary experiment, bearing a remote resemblance to the sort of situation that Maier used. To invite comparison, the same form will be used here. Four goal boxes are used, R, U, X, and H. R is connected with every other box and U is connected to X (Fig. 2). The rat is then trained to run from R to X for a reward of food. Following this it is trained to go from U to X for food reward, from R to U for a water reward and from R to H for a water reward, in that order. We may assume that after learning the tendencies to go to U and H when thirsty on R are equal for the rat. In the crucial trial the animal is placed hungry on R with the path to X blocked. There is then only one way to X and this is through U. The rat has never been this way before in any one trial. However, it appears plausible that rats would synthesize their partial experiences. They would run towards U, and then to X, more frequently than to H.

Assume that four rows of links have been occupied by analysers during training, each consisting of four links. (This number is arbitrary and adopted for the sake of simplicity.) These may be represented as shown in Fig. 3. The first link has an analyser which responds to food attached to its first link, then an analyser set off by a feature on platform *X*. There follows *UX*, an analyser set off by a landmark between *U* and *X*, and then *U*, an analyser 'representing' platform *U*. The other rows in the diagram are to be similarly interpreted.

When the animal is made hungry, rows 1 and 2 will be excited according to rules 2 and 3 (rule 2: 'The first links in these rows are set into activity by a feature of the internal medium in proportion with the magnitude of this feature (such as dehydration)'; and rule 3: 'The first link excites its neighbour (since any link which is excited, excites the next link) and so on until the whole row is excited'). This excitation will be transmitted to row 3 via the common analyser *U* and to row 4 via water analyser. (Rule 14: 'When an analyser is connected to two or more links at once, a small fixed proportion of excitation will be passed through it from one link that may be excited to the others to which it is fixed.') We may neglect for the purpose of this explanation the excitation transmitted via the analyser *R* to rows 3 and 4 as the excitation in the link to which analyser *R* is attached in row 2 will cease as soon as *RX*, a cue between *R* and *X*, appears (rule 8), when the animal is placed on the platform *R*, assuming that *RX* is in front of the block. The amounts of excitation at the links to which analysers *RU* and *RX* are attached are unequal, as more links intervene between *RU* and the source of excitation than *RX*, and *RH* than *RU* (rules 12 and 13). The animal will accordingly approach *RX* (rule 13). As there is a delay in the appearance of *X* owing to the block, excitation at the link to which the analyser *RX* is attached will decrease (rule 11) and the link to which analyser *RU* is attached will begin to determine output. Hence the animal will take the path leading to *U* and thence to *X*.

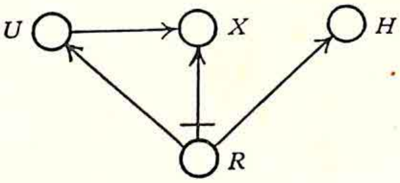

Fig. 2.

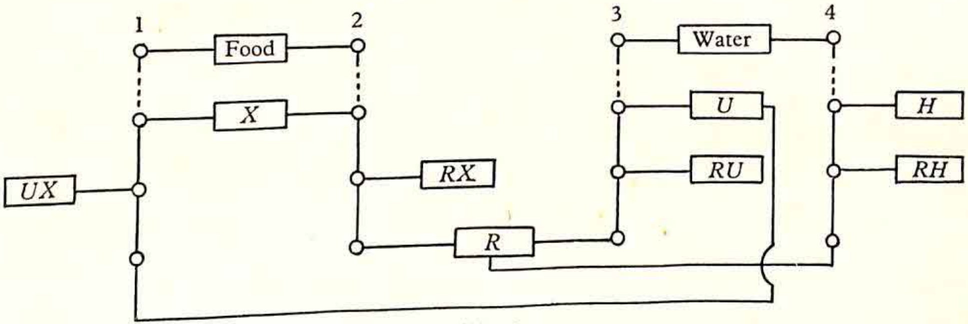

Fig. 3.

From a comparison of this explanation with that of the experiment on p. 122 it will be seen that success in solving the problem should only occur if there is a sufficiently large difference in the number of links between U and RU on one hand and U and RH on the other. Translated into experimental terms the platforms should be large, and well furnished with many cues, so that the 'psychological' distance between the water cups and the two alleys is made large. Unless this condition is met, success in Hull's 'conventionalized version' would not be expected to occur.

Unfortunately this prediction does not differentiate between this theory and Hull's. Hull's explanation of his own fiction is fallacious.

It should be clear from the preceding analysis that the hypothesis should explain Blodgett-type latent learning. Here the rat is allowed to explore a maze without a reward. It is then rewarded in one place in the maze and then inserted in it at a different point. The rat can then apparently find its way to the place of reward, perhaps without an error.

The rat, according to the hypothesis, should in its random explorations record the order of cues preceding something with which it was familiar (that is, a cue which 'represented' an analyser already attached to a row). Some of the cues would be noticed again on subsequent explorations. When the animal is motivated and given a reward in the proximity of a distinctive cue, the excitation due to this motivation will flow through the analyser to any other rows of links to which it is attached and from these to any further rows of links through analysers which they have in common. The animal will then find its way to the reward from any point in the maze, providing that the cues which it picks up where it is placed are represented by analysers connected to links which are indirectly influenced by excitation emanating from the link to which the reward analyser is attached. This may be seen by inspecting Fig. 4. (Only common analysers are indicated.) Suppose that excitation is being introduced at X from row 5. All the rows 1–4 will be excited. Also suppose that the animal is so placed in the maze that it is receiving stimulation from an analyser attached at J. The analyser attached to the next link on row 2 will then fire, switching off the excitation both to the link to which J is attached and to row 1. When, while approximating itself to the cue which had set off this analyser, it picks up signals from the next, row 2 will also be switched off as also the initial part of row 3. Further approximation to cues represented by analysers attached to links still excited can only bring the animal nearer the source of excitation, and so to the reward. The mechanism would operate in the same favourable way if the animal were placed at

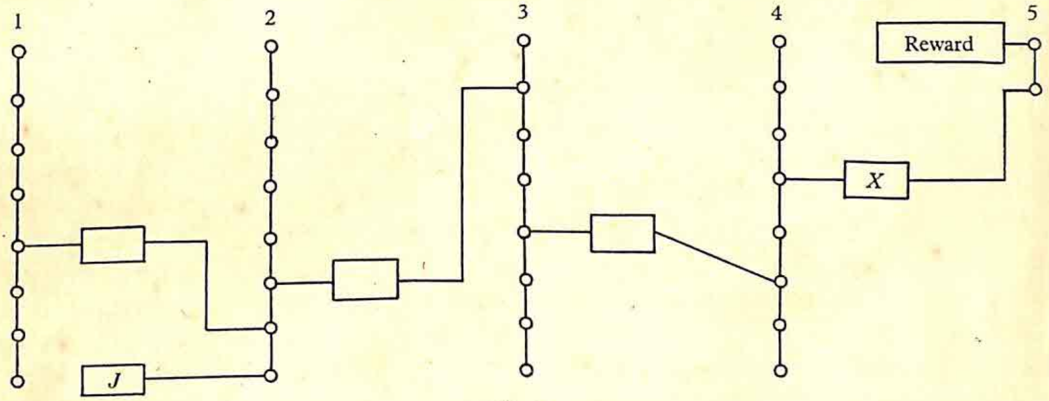

Fig. 4.

X with *J* as the goal. If excitation is introduced into a row via two different analysers, and this gives rise to conflict, it will be resolved by the operation of rules 12 and 13.

From the foregoing it would appear that the animal should find its way to any reward which it noticed, whatever its motivational state. This, however, is a superficial deduction. Though the information may have, in fact, been registered, it may not in some situations lead towards a correct performance. For instance, many experiments are done in such a way that a prominent identical cue is found in both the goal boxes between which the animal must choose on the critical trial (Kendler & Mencher, 1948; Grice, 1948). An animal is made hungry and then run to two goal boxes containing food. In one of these water is also placed. Then the animal is made thirsty. It fails to choose the relevant side. Fig. 5 shows a schematic representation of the hypothetical mechanism in the animal at the time of the choice in the critical trial. Excitation originating in row 3 owing to dehydration is conveyed to row 2 by the common analyser. Row 2 is excited and this excitation transferred to row 1 via the analyser common to rows 1 and 2. As there has been an equal amount of reward and practice on rows 1 and 2, the only thing which could decide that row 2 should control output is the slight disparity in the number of links (and analysers) through which the excitation has to travel in rows 1 and 2. It is assumed that such a difference is too small to eventuate in the correct performance.

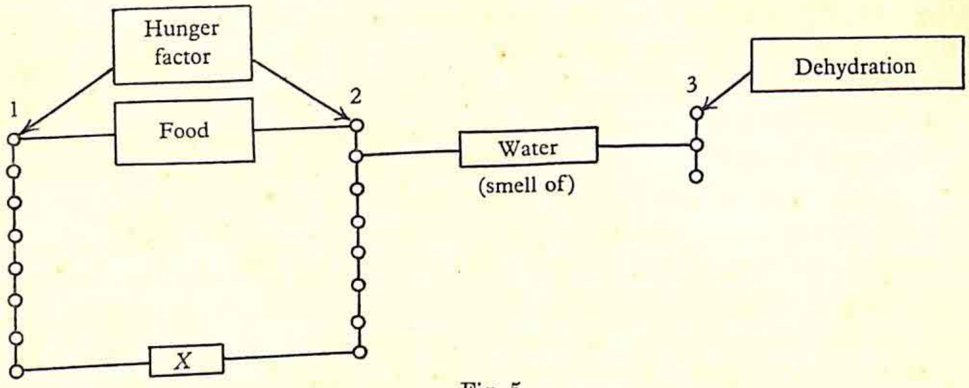

Fig. 5.

Such an explanation of the failure to find latent learning in the above situation may not seem plausible. But it can be checked in one or two ways. For instance, if the water analyser was attached farther from the beginning of row 2, there would be a greater disparity between the number of links between analyser *X* on rows 1 and 2, with the balance more heavily in favour of row 2. Such experiments (with food as the irrelevant incentive) have in fact been done, one by Thistlethwaite (1952) and the other by McAllister (1952), with the result deducible from the present theory. They found that rats showed a significant tendency to choose the relevant side when they were made hungry if the relevant reward (water) was widely separated spatially from the irrelevant reward (food) in one of the arms of the maze. The difference between the results of Spence, Bergmann & Lippitt (1950), and Maltzman (1950) also supports this explanation.

There is another way of testing the hypothesis in its application to the present instance. That is to have two differing relevant incentives in the two arms of the maze with the irrelevant incentive only associated with one. There would thus be no common analyser to transmit excitation from row 2 to row 1, in the way the 'food' analyser does in the diagram. The animal should then make the correct choice. The argument in the above deductions rests on the assumption that there is only a relatively small number of other common analysers in the two arms of the maze past the choice-point (except those associated with the reward in some instances). The argument rests on a ratio of analyser to link resistance ratio. Obviously the greater the number of common features in the two alleys the less likely the animal is to choose the relevant path. Hence the animal's success will depend on where in the sequence it picked up similar cues (which is usually left to chance, but which could be varied) and hence on the dissimilarity between the two alternative paths.

Another type of experiment where animals have failed to show any evidence of latent learning is where the relevant reward is placed in one goal box and the irrelevant reward in another (Spence & Lippitt, 1946; Kendler, 1947*a*). If the animal is made hungry and shown water in one alley and then rewarded with food in the other, it will still tend to go towards the food side when it is made thirsty. Assuming there are some similar cues common to both alleys, a portion of the 'thirst' excitation arriving at the row of links 'representing' the alley leading to the incentive originally used as reward will be transmitted by a set of links with much lower thresholds. For according to rule 12, the combination of high excitation and reward in this row should have reduced the 'resistance' to the passage of excitation considerably in this row. Thus, though it receives less excitation to begin with, any excitation passed on by it should receive less 'attenuation'. Hence the animal should either behave randomly, fail or succeed according to the intensity of motivation on the rewarded food trials, the number of times it was food-rewarded and the similarity of cues on either side. Increasing the water motivation on the critical trial should make no difference to its choice. If there had been an equal (or zero) amount of reward on both sides, the animal should choose correctly unless there were strongly similar cues along the two different paths (Kendler, 1947*b*, Meehl & McCorquordale, 1948). Strong irrelevant motivation should not in itself hamper the subsequent utilization of information provided the motivation has not been rewarded. This deduction is borne out by Strange's (1950) experiment. Animals which had been deprived of water for 22 hr. were given an equated amount of training to two goal boxes in a U maze. One goal box was empty, the other contained food. When they were made hungry and allowed to choose, they gave evidence of having learnt the location of the food.

In case it is felt that the hypothesis presented is entirely *a posteriori*, here is a rather curious prediction. Assume there are two rows, both equated for 'resistance'. Further assume that they both share an analyser X, but that this analyser is in different ordinal positions on each (Fig. 6). Then if we excite both rows simultaneously and equally the animal, being given a free choice, should choose the path which contained the cue corresponding to analyser X nearer to the choice-point, though invisible from it. Such an experiment could be performed in the same sort of conditions as a spontaneous alternation experiment.

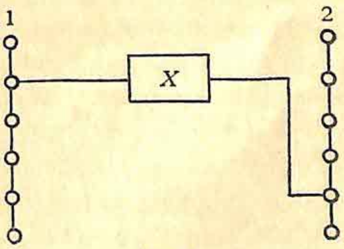

Fig. 6.

The hypothesis put forward is, however, exposed to numerous objections. It will be said that it would predict no learning in a situation where the animal has to take a different path to the same goal box according to its motivational condition (though learning to go to different goal boxes should be rapid). That animals find the second easy and the first inordinately difficult is indeed the case, but we cannot equate slowness with complete inability. The relevant experiments have been done by Hull (1933) and Leeper (1935). However, there is a possible way of escape for the theory as we assume that the excitation travelling from one row to the other is not transmitted in its entirety, and that the proportion transferred remains fixed. The proportion of excitation transmitted directly from link to link would be expected to rise after each rewarded trial. There would thus be a progressive increase of the absolute difference between the competing links (at the choice-point), and slow learning. On this view the speed of appearance of the correct response would be a function of the similarity of the goal boxes, if we were using two.

It is doubtful, however, whether this explanation could cover all forms of conditional learning. Neither will it account for many other phenomena customarily included under the heading of higher mental processes such as delayed reaction, double alternation, abstraction and perception of relations. However, it would be possible to extend the hypothesis or to put forward a set of complementary hypotheses. This has already been done to deal with a special case of relational perception (Deutsch, 1955). It is, however, unwise to embark on various speculative elaborations to cover remote and complex cases before the more basic ideas in the theory have been tested and verified to some degree. The present modification and extension of the previous theory was prompted by the fact that one of its important predictions has now been confirmed in a study (as yet unpublished) by N. S. Sutherland. The prediction is that spontaneous alternation is goal alternation as well as stimulus alternation. Hence, if an animal is equally rewarded and trained on two sides of a T-maze where the goal boxes are out of sight at the choice-point, then if it is directly placed and fed in one goal box, it should tend to run to the other when it is placed again at the entrance of the maze and given a free choice. This tends to confirm the notion, unique to the theory, that 'motivational' excitation is transmitted through units underlying acts nearer the goal to those farther away from it, and that stimulation, far from increasing this excitation, actually severs its flow.

The author wishes to thank Mr A. J. Watson and Mr J. von Wright for many valuable discussions, Dr Kay for his wise advice and encouragement and Mr Mandelcwajg for his criticisms of a draft of the paper. He is also indebted to the Medical Research Council who generously enabled the author to carry out the work on singing.

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SEX DIFFERENCES IN PREFERENCES FOR SHAPES: A CROSS-CULTURAL REPLICATION

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A shape-preference test was designed by McElroy on the basis of 'a possible corollary of psychoanalytic theory' and administered to Scottish schoolchildren. In accordance with predictions there was a significant tendency, increasing with age, for boys to prefer round shapes (disguised female symbols) and for girls to prefer rectilinear ones (disguised male symbols). McElroy suggested alternative hypotheses that might, in his view, account equally well for the effects obtained.

In the present study McElroy's main alternative hypotheses were examined and it appeared that, as formulated by him, they could not be used to predict results in Accra. It was shown, however, that the psychoanalytic hypothesis did permit a definite prediction: in a cultural situation where there is less repression, one would expect sex differences to be smaller. The test was given in a modified form, which made more detailed analysis possible. A greatly reduced, but still significant, sex difference was in fact obtained. Age changes, contrasting with those found in Scotland, were also shown to be consistent with the psychoanalytic hypothesis. It was further suggested that some of the specific Freudian interpretations of symbols may have to be reconsidered.

I. CULTURAL CONTRASTS

In a recent paper, to which reference should be made for full details, McElroy (1954) described a shape-preference test and reported the results of its administration to Scottish schoolchildren. He discussed several hypotheses considered relevant in connexion with an obtained sex difference. The aim of the present study was to discover if such a sex difference exists in a very different cultural setting; and if this was the case, to test some of the hypotheses advanced.

At the outset a brief sketch of those cultural factors that appear relevant in the present context will be attempted. The work was carried out in the Accra region where the Ga tribe, to which most of the sample belongs, is concentrated. Their traditional family structure is of a form rarely encountered in any other part of the world. Men and women live in separate households. The wife or wives in turn supply the husband with food and visit him in his room at night. Children below puberty live in the women's compound. At puberty boys move to their father, whilst girls remain with their mother until they are married. This pattern is gradually changing, but there is evidence that it still holds for a considerable proportion of the population.

The writer has occasionally observed sex-play among small children; adults who discover it happening will discourage this, but without any obvious signs of disgust and moral indignation. Although in the past pre-marital chastity was apparently stressed, it is the exception rather than the rule to-day.* The sterner rules of Christian sexual morality are not very firmly entrenched, as is evident from the large numbers of nominal Christians who enter polygynous marriages. One may say with confidence, therefore, that there is likely to be far less repression among the Ga than in Scotland.

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* Field (1940), the chief authority on the Ga people, wrote as follows (p. 39): 'If the bridal night proves to the husband that the bride was a virgin he takes to her parents, on the following morning, a piece of new calico stained with the blood of the ruptured hymen. If he finds that she is not a virgin he has the right to demand of her parents a goat to be slaughtered to stain the calico. But in recent years this custom has almost died out, for, I am told, "not one girl in a thousand is a virgin". Schoolgirls are said to be the least chaste of all and frequently sell their favours for small sums of money.' There is no doubt that the social changes brought by the War, especially the presence of foreign servicemen, have greatly enhanced the trend in this direction.

In connexion with McElroy's 'stimulus generalization' hypothesis it is also worth noting that boys have extensive opportunities of seeing the exposed breasts of girls and mature women.

Turning to another relevant topic it can be stated that there are hardly any indigenous ornaments, except perhaps on pottery, and these are invariably round; the same is largely true of the decorations on imported cloths. In general, few Gold Coast Africans appear to be interested in pictures as such, apart from a small Westernized minority. In most ordinary African houses in Accra one can usually find photographs of family members or portraits cut from magazines, but hardly ever representations of any other objects.

As against this some Western influences, acting chiefly upon schoolchildren, must be considered. First of all 'art' is taught at schools; shields are extensively used as sports trophies in both boys' and girls' schools; and lastly there are several flourishing Scout troops in Accra, so that the Scout emblem is fairly well known among children and young people.

II. McELROY'S HYPOTHESES AND POSSIBLE PREDICTIONS

Looking at McElroy's main hypotheses in the light of the above, it becomes apparent that they are of very unequal value. In particular, he claims that 'the well-established principle of stimulus generalization' could have been used to predict the sex differences. A plausible contention at first sight, but it will not bear closer examination.

Examples are quoted of generalization of fear responses exhibited by rats 'from one food cup to a highly similar drinking cup', and by cats from one particular food-box to 'all food-boxes and associated configurations'*; then, without any transition, follows a reference to 'associated configurations of the male and female form'. This seems rather an unwarranted jump, considering among other things the circumscribed nature of the experimental work on stimulus generalization, and the findings concerning generalization gradients; these show a progressive decline of response as the degree of similarity between the stimulus objects decreases (cf. Hovland, 1951; Vinacke, 1952). The phrase 'associated configurations of the male and female form' disguises the fact that the common elements shared by women and curved drawings on the one hand, and men and rectilinear drawings on the other, are severely limited. Furthermore, the attempted explanation of age-changes in preferences by means of this hypothesis as 'generalization of fear responses' connected with 'female adolescent development' is too patently *ad hoc* to be very convincing.

In the present case the hypothesis did not seem to permit an unambiguous prediction. With reference to the boys it might be argued that the more frequent exposure to the primary stimulus (i.e. female breasts) would lead to wider generalization and therefore to a greater sex difference than that found in Scotland; but it is equally possible that 'negative adaptation' will occur, or in other words that the boys will become accustomed to and blasé about the sight of female bosoms, with a consequent lessening of generalization; if the latter holds, the sex difference should turn out smaller than the Scottish one. It will be seen that the formulation is so loose that no clear-cut consequences can be deduced, and a hypothesis which is incapable of being falsified is worthless.

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* A somewhat more relevant example might have been Watson and Rayner's (1920) experiment; it will be remembered that they conditioned a child to fear a rabbit and found a generalized fear of dogs, fur coats and Santa Claus masks.

A similar criticism could be made of McElroy's references to identification or 'empathy'. He says (p. 212): 'Considering the direction of the deviation, it is apparent that identification responses have not provided the principal basis for preference, although it is likely that they have contributed towards the total experimental results.' No evidence whatever is offered to show that identification did in fact operate.

In contrast with this the psychoanalytic hypothesis can be made the basis for a definite prediction; there is less repression among the Ga, hence sex differences will be less pronounced than in Scotland.

III. SAMPLE AND TEST ADMINISTRATION

The main experimental group consisted of 858 boys and girls from eight different Middle Schools in Accra. These schools are approximately equivalent to the former Senior Elementary schools in Britain. In each the top and bottom forms were tested in order to obtain an adequate age spread. In the absence of compulsory registration reliable information about ages is impossible to obtain. The pupils' own estimates were therefore accepted; although subject to considerable error in individual cases they do make it possible for the sample as a whole to be at least roughly divided into an older and a younger group. For both boys and girls the modal age estimate in Form I was 13 years and in Form IV 16 years, with a total range from 11 to 19 years.

Table 1

	Males		Females		Proportions of preferences					
					'Female' shapes		'Male' shapes			
Item	*A*	*B*	*A*	*B*	Male	Female	Male	Female	χ^2	*c*
1	153	275	137	293	0·643	0·681	0·357	0·319	1·35	1·16
2	189	239	101	329	0·442	0·235	0·558	0·765	36·93	6·08
3	341	87	300	130	0·797	0·698	0·203	0·302	8·22	2·87
4	269	159	235	195	0·629	0·547	0·371	0·453	5·71	2·39
5	240	188	254	176	0·561	0·591	0·439	0·409	0·79	0·89
6	336	92	266	164	0·785	0·619	0·215	0·381	23·50	4·85
7	224	204	221	209	0·477	0·486	0·523	0·514	0·04	0·20
8	96	332	145	285	0·776	0·663	0·224	0·337	10·74	3·28
9	79	349	94	336	0·815	0·781	0·185	0·219	0·91	0·95
10	91	337	83	347	0·787	0·807	0·213	0·193	0·19	0·44
11	63	365	83	347	0·853	0·807	0·147	0·193	1·68	1·30
12	194	234	222	208	0·453	0·516	0·547	0·484	3·40	1·84

$\chi^2 = 23{\cdot}89$; $r_p = 0{\cdot}05$; $c_{mf} = 4{\cdot}89$.

Classrooms are often greatly overcrowded, so that a certain amount of copying cannot be prevented; no mixed classes were therefore included in the sample.

For the subsidiary experiment two further schools, one boys' and one girls', were used. As the age distribution did not matter in this case, pupils in all four forms were included, totalling 278 boys and girls.

The actual drawings were taken over from McElroy by careful tracing, with only one minor modification: the horizontal bar across the lower part of the fleur-de-lys was left out in order to avoid too specific a representation of the Boy Scouts' badge. The general layout, however, was altered in an attempt to secure data which would permit the checking of some of his hypotheses. All pairs of drawings were arranged in a single column on the centre of a foolscap sheet in such a way that rectangular blank spaces were left on either side of each pair of drawings.

The first part of the instructions was closely similar to those given by McElroy, except that the subjects were asked to put a ring around the picture they preferred and to cross out the one they liked less; this seemed the safest way of avoiding misunderstandings due to language difficulties. When everybody in the class had completed recording their preferences, they were invited to write by the side of each picture of what it reminded them.

IV. RESULTS OF MAIN TEST

The preferences of boys and girls for 'male' and 'female' shapes are shown in Table 1, laid out in exactly the same way as McElroy's table with the Scottish results and computed on the same basis.*

Significant sex differences emerged for only five out of the twelve items; one of these, no. 4, happens to be the single non-significant item for the Scottish sample. The pooled results of all items produced a χ^2 of 23·89, corresponding to a critical ratio c_{mf} of 4·89. This represents a rather low level of significance in comparison with the Scottish results, where a χ^2 of 154·56 was obtained, which is equivalent to a c_{mf} of 12·43. The overall fourfold point correlation r_p was 0·13 in Scotland and 0·05 in Accra.

The mean male test 'score' was 8·00 preferences for disguised 'female' configurations or 4·00 preferences for disguised 'male' configurations; the standard deviation of the male 'scores' was 1·72 and the standard error 0·083. The mean female test 'score' was 4·57 preferences for disguised 'male' configurations or 7·43 preferences for disguised 'female' configurations; the standard deviation of the female test 'scores' was 1·79, and the standard error of the mean 0·086.

The relationship between age and preferences was again analysed by McElroy's method. For reasons explained above the age division had to be shifted upwards by two years. The number of preferences for 'female' shapes are given below:

	Male		Female	
	No. of subjects	No. of preferences	No. of subjects	No. of preferences
Below 14 years	104	820	114	765
14 years or above	324	2603	316	2428
	$c_d = 0{\cdot}81$		$c_d = 5{\cdot}29$	

There is a striking contrast here with McElroy's findings; whilst the Scottish boys and girls both diverged significantly with rising age, the Accra boys did not change and the Accra girls converged significantly towards the boys' preference pattern.

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* There appear to be several computing errors in McElroy's numerical results. None of them is large enough to affect his general argument, but for the purpose of comparison corrected figures will be used.

First, he arrived at the overall χ^2 value of 169·85 by adding up the individual χ-squares for item nos. 1–12; this is quite in order, provided the degrees of freedom are also added up; but he proceeds to use the square root of χ^2 thus obtained as an estimate of the critical ratio, which can only be done with one degree of freedom.

The writer did not work out the fourfold point correlations for each item, as they convey hardly any additional information. The r_p for all items pooled was computed from McElroy's data as being 0·13; it is not clear how he arrived at an 'average' figure of 0·14.

Finally, the standard errors of the means are given wrongly; it seems that he divided the standard deviation by the square root of 12, i.e. the number of items, whereas it should have been the square root of the numbers of boys and girls respectively. The standard error of the mean should therefore read 0·088 for the male 'scores' and 0·080 for the female ones.

It is only fair to add that it was possible to spot these errors only because McElroy made his numerical results available in full, a practice that deserves wider currency.

V. SUBSIDIARY EXPERIMENT AND CONTENT ANALYSIS

Comparison of the Scottish and Accra preferences of both sexes combined shows that 'female' choices are nearly twice as frequent in the latter (37·2 % as against 64·3 %). In trying to find the factors responsible for this difference the identifications of the drawings were examined, i.e. the responses to the question 'of what does this remind you?' A strong association between preferences and naming was noticed; in other words, non-preferred pictures were rarely identified. This seemed congruent with the findings of Carmichael, Hogan & Walter (1932) on the influence of naming upon recall, and the 'effort after meaning' often encountered in experiments on perception (cf. Vernon, 1952). The causal connexion thereby suggested is that certain drawings are preferred *because* they are more meaningful to the subject than the other member of the pair. Thus it would seem to follow that the subjects found it more difficult to name rectilinear drawings than round ones, presumably owing to the fact that round objects are more common in their cultural setting than square ones.

In order to check this conclusion all the pupils in one boys' and one girls' school were given the test, but without any mention of preferences; they were merely asked to identify as many of the drawings as they could. It was predicted that a larger proportion of the 'male' pictures would be left unidentified. This proved to be the case, as is shown below:

		Percentage of shapes unidentified	
	No.	'Male'	'Female'
Boys	145	41·6	24·2
Girls	133	41·0	22·7

At the same time it is clear that the sex difference cannot be accounted for in the same way, as the proportions are closely similar for both boys and girls.

The next step was the analysis of the verbal labels attached to the shapes in the course of the main experiment. This was done in an attempt to investigate McElroy's suggestion that different associations might be called up in boys and girls viewing the same shapes.

A broad classification of responses is given in Table 2 for items nos. 2, 3 and 6; reasons for picking these three are as follows: no. 2 reached the highest level of significance; with no. 6 *A* and *B* 'crown' was the dominant response of both sexes; no. 3 contains the wavy line, which will be referred to later. The remaining two significant items present no new features.

Examination of the material in Table 2 indicates the small likelihood that the sex difference could be accounted for by this factor, which is perhaps better described as that of 'familiarity'; the term 'associations' seems rather too wide, as the direction of association will itself be determined at least in part by dynamic unconscious factors such as repression.

There were only two small groups of verbal labels used by one sex and not the other; these were 'brooch', in item no. 2, mentioned exclusively by girls, and 'boxing gloves', in item no. 3, restricted to boys. There is no evidence of any important sex difference in familiarity as regards the large majority of the objects cited.

This is not to deny, of course, that such a factor did operate in individual cases. An example is the boy who chose the pointed 'crown' and wrote that it resembles the one on the 'Cow and Gate' tin, which is quite correct.*

McElroy also thought that girls' greater intolerance of inaccuracies, for which further proof would be required, might have influenced the results. A rather superficial effort was made to find out about this by asking the boys and girls in several classes to explain their preferences after they had finished. As was to be expected they found this a rather difficult task. The usual response consisted of a mere reiteration: 'I like this because it looks like...'. For the present purpose it is only necessary to note that not a single subject attributed his or her preference to the fact that one shape was better drawn than the other. What was said in the introduction about the relative lack of interest in pictorial representation points in the same direction.

Table 2. *Percentage distribution of different types of objects suggested by the drawings*

	Boys		Girls	
	11–13 years	14 and over	11–13 years	14 and over
Item no. 2*A*: 'female'				
Shield	38	32	10	9
Badge, emblem, crest	4	5	2	7
Other	3	3	3	4
Item No. 2*B*: 'male'				
Flower, tree, leaf	16	21	55	38
Banana, maize	20	4	9	16
Boy Scout badge	13	27	16	12
Badge or crest	5	6	4	3
Brooch	—	—	1	8
Other	1	1	1	2
	100	99	101	99
Item no. 3*A*: 'female'				
Snake	49	69	46	63
Worm, caterpillar	2	2	4	3
Rope, chain, thread	17	3	1	1
Pattern, design, 'art'	6	3	—	5
Other	4	5	2	3
Item no. 3*B*: 'male'				
Drawing, pattern, 'art'	6	4	29	7
Zigzag	7	6	5	6
Letters 'V' or 'W'	4	3	7	8
Other	5	5	6	3
	100	100	100	99
Item no. 6*A*: 'female'				
Crown	72	61	52	64
Hat, cap	7	9	4	1
Boxing gloves	1	4	—	—
Other	—	1	3	2
Item no. 6*B*: 'male'				
Crown	19	17	34	32
Hand, gloves	1	3	3	—
Other	—	5	4	2
	100	100	100	101

VI. Interpretation of the sex differences

Of the several major hypotheses advanced by McElroy only the psychoanalytic one appears to survive the process of elimination pursued in the present investigation. But there is yet one more problem to consider, namely the decline in the older girls' preferences for 'male' shapes. The writer unfortunately cannot pretend that he was able to anticipate this—on the contrary, it came as a considerable surprise. However, brief reflexion showed that it *could* have been predicted on the basis of the psychoanalytic hypothesis.

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* It appears that the boy had recently been asked to purchase a tin. There is a widespread belief that Europeans always feed their infants on artificial milk, and a tendency among literates to follow what is mistakenly regarded as the better practice.

From the writers' general studies of adolescents in Accra it would seem that a considerable proportion of young people have sexual experiences during their 'teens, and there are indications that this occurs with girls earlier than with boys. When the present problem arose the writer discussed this matter with several experienced welfare officers, probation officers and doctors in order to obtain an independent check on his impressions; their judgements coincided almost unanimously with the above conclusions.

The general trend may thus be interpreted as follows: for the boys, taken as a group, the heightening of repressed, and probably also suppressed, affect with increasing age is counteracted by partial* outlets which roughly balance the former; full sexual activity mostly occurs at a later age than that of the majority of the boys represented in the sample. In the case of the girls full outlets are achieved after about the age of fifteen, involving a lessening of repressed or suppressed affect, which is withdrawn from the symbolic and transferred to the actual objects.

It has to be admitted that this line of theorizing is *ex post* and founded on indirect inference, in the sense that the decline of repression or suppression in girls and its maintenance in boys has been assumed rather than demonstrated; but although it is speculative to some extent, the conclusion appears to fit the facts better than any alternative hypothesis.

One further statement can be made with confidence: whilst the results are consistent with the general psychoanalytic theory of symbolism, they contradict in at least one particular Freud's assertions about the meaning of symbols. Among those preferring it, there was a significantly larger number of boys than of girls who called the wavy line in item no. 3 a 'snake'; but Freud (1922, p. 167) maintained that the snake is the classical male symbol. Thus it is possible that McElroy's broader formulation concerning curved versus rectilinear forms may have more general validity, and that some of Freud's other specific interpretations which contradict it might turn out to be either false or at least misleading.

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* The term 'partial', used with reference to the group, leaves it indeterminate whether a majority of the boys obtain some limited sexual satisfactions, or whether a minority attain complete ones. There is insufficient evidence to decide this question.

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A NOTE ON THE SCALE PRODUCT AND RELATED METHODS OF SCORING ATTITUDE SCALES

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Attitude scales constructed by Thurstone's method of equal-appearing intervals may also be scored by Thurstone's method or by Likert's or by a combination of the two. Some possible advantages of a combined method are described.

Three variants of such a combined method are distinguished, and their characteristics discussed. In the case of two of these, no evidence is found to show that they possess any advantage over Likert's method of scoring, and some evidence seems to indicate that they do not. For the third (the Scale Product method of Eysenck & Crown, 1949), higher reliability is claimed by its authors, but insufficient information is available about the details of the method for this claim to be tested.

I. THURSTONE'S AND LIKERT'S METHODS OF SCORING

It has long been recognized that both the Thurstone and Likert methods of scoring attitude scales suffer from certain weaknesses, and several attempts have been made to develop a new method which would combine the advantages and avoid the defects of both. This note will not be concerned with alternative methods of *constructing* attitude scales; a good case appears to have been made for the superiority of the Thurstone technique of equal-appearing intervals (Ferguson, 1939), and all the scales to be discussed were constructed by that technique.

The Thurstone method of *scoring* seems more open to question. The respondent is asked to mark those items on a list of attitude statements with which he agrees, and the median scale value of these items then becomes his score. This dichotomization of the items into those 'agreed with' and 'disagreed with' is somewhat crude and fails to extract the maximum information. Not only is the respondent likely to agree to quite different degrees with the various selected items on which his score is directly based; in the case also of the items which he does not mark he would presumably express *degrees* of disagreement if the form of questionnaire permitted.

One theoretical weakness of the Likert system of scoring is equally obvious. Of two items one may have a Thurstone scale value of 9, and another, much less extreme, a scale value of 6; yet under the Likert system approval (or disapproval) of these two items will earn the same mark.

The respective advantages and disadvantages of the two methods seem, therefore, to be as follows. The Thurstone method accurately weights each item, although it does not allow for shades of agreement or disagreement; the Likert method permits a number of possible responses to each item, ranging, for example, from 'strongly agree' to 'strongly disagree', but gives equal weight to items which should ideally receive differential weighting. On balance, Likert's technique appears slightly preferable, both because scores are easier to compute and because it has been shown by Likert, Roslow & Murphy (1934) to produce rather higher coefficients of reliability. It is obviously tempting, however, to try to produce a type of scoring which will take into account both the scale values of items and the graded responses of individuals.

Three such attempts are those of Likert, Roslow & Murphy (1934), Eysenck & Crown (1949) and Castle (1953). All these newer techniques combine features of Thurstone's and Likert's methods, but they differ slightly in details of procedure and in the results claimed for them.

II. THURSTONE-WEIGHTED SCORING

(i) *General characteristics*

Likert, Roslow & Murphy describe what they call 'Thurstone-weighted' scoring as follows:

> Another method which is in a way a combination of the Thurstone and simpler (i.e. Likert) methods was also tried.... The same statements were used in this scoring as were used in the simpler method, but instead of assigning 1–2–3–4–5 to the respective alternatives, the values assigned were based upon the scale values of the Thurstone method. The 'strongly agree' alternative of each question was given the value shown by the Thurstone key for that statement.
>
> The 'strongly disagree' alternative of each statement was given a value equal to 11·0 minus the value of the 'strongly agree' alternative for that particular statement. The question mark was always given the value of 5·5 and the 'agree' alternative for each statement was given a value midway between 5·5 and the value of the 'strongly agree' alternative for the same statement. The 'disagree' position was similarly scored.

Certain features of the Thurstone-weighted technique, as described by Likert, Roslow & Murphy, merit further attention. First, the scoring of the 'strongly agree' and 'strongly disagree' responses to each item is weighted according to the Thurstone scale values of the items, and the same principle applies to a lesser degree to the 'agree' and 'disagree' responses, but the 'uncertain' responses all receive the same score, regardless of the scale value of the item concerned. This is probably a slight theoretical weakness, since a subject who is uncertain about a statement at the positive end of a Thurstone-constructed scale may well be manifesting a very different attitude from another subject who is uncertain about a statement at the negative end, yet with the Thurstone-weighted technique as with the Likert, uncertainty on either of the two items will contribute the same mark to the subject's total score.

Another characteristic of this kind of scoring is worth noting for purposes of comparison. The method of computation ensures automatically that, as with Likert scoring, the *sum* of the five weights is the same for each item.

(ii) *Reliability*

Likert, Roslow & Murphy scored several Thurstone-constructed attitude-scales both by the ordinary Likert and by the Thurstone-weighted techniques, and found high agreement between the two sets of scores, with correlations between +0·95 and +0·98. (The correlations of Thurstone-weighted scores with ordinary Thurstone scores were markedly lower, ranging from +0·83 to +0·87.) The respective reliability coefficients resulting from the Likert and the Thurstone-weighted scoring techniques were not specifically compared by the authors, who were satisfied by the high correlations between the two that little or nothing was to be gained by using the considerably more laborious Thurstone-weighted method. It is possible to extract and compare some of the reliabilities from data provided in separate tables in the article. This comparison (and tests of significance) confirm the conclusions of Likert, Roslow & Murphy. None of the differences in reliability, as between the two methods of scoring, is statistically significant (see Table 1).

III. SCALE PRODUCT SCORING

(*i*) *Eysenck & Crown's description*

The above comparison of reliabilities is of particular interest in view of the claim made for the rather similar Scale Product technique devised by Eysenck & Crown (1949), who describe their method as follows:

Table 1

N	Attitude scale no.	Likert scoring. Split-half reliability	Thurstone-weighted scoring. Split-half reliability
61	23	+0·91	+0·86
65	30 (groups D and E)	+0·89	+0·88
50	30 (group F)	+0·89	+0·90

Table 2. Original Weights

Item no.	Scale position	Strongly agree	Agree	Uncertain	Disagree	Strongly disagree
1	3·7	5	5	4	3	3
2	7·8	1	3	4	5	7
3	4·9	5	4	4	4	3
4	8·5	1	2	4	6	7
5	1·3	7	6	4	2	1
6	2·0	7	5	4	3	1
7	4·6	5	4	4	4	3
8	9·2	0	2	4	6	8
9	2·9	6	5	4	3	2
10	6·2	3	3	4	5	5
11	0·5	8	6	4	2	0
12	6·9	2	3	4	5	6
13	7·3	2	3	4	5	6
14	1·0	8	6	4	2	0
15	8·1	1	3	4	5	7
16	1·7	7	6	4	2	1
17	6·5	3	3	4	5	5
18	8·9	1	2	4	6	7
19	2·5	7	5	4	3	1
20	5·3	5	4	4	4	5
21	5·7	3	4	4	4	5
22	3·4	6	5	4	3	2
23	9·7	0	2	4	6	8
24	4·1	5	5	4	3	3

> In this method, the weight given to the various responses to each item by the Likert method is multiplied by the scale position of the item as found in the Thurstone method. The rationale of this procedure is obvious. As an example, let us take two items, both of which are answered 'strongly agree', and would therefore score 5 points on the Likert system. One of these items, say, is mildly anti-Semitic, the other extremely so. (The relative Thurstone scale positions might be 6 and 10, where 5 denotes neutrality.) Clearly the endorsement of the second item is a much stronger indication of anti-Semitism than the endorsement of the first, and should consequently be weighted much more heavily. This weighting is accomplished automatically by multiplying the Likert weight(s) by the scale position (6 and 10 respectively), so that the respective weights would be, according to the method of Scale Products, 30 and 50.
>
> This method gives rather unwieldy numbers, and can be improved by dividing all products by a constant, and rounding off. This brings the scale products down to manageable proportions. The actual values finally selected for the anti-Semitism scale are given in table 2, under the heading 'Original Weights'.

Eysenck & Crown's table 2 is reproduced as Table 2 above.

(*ii*) *Reliability*

Eysenck & Crown then report that this Scale Product method of scoring, when applied to their scale of anti-Semitism, produced a reliability of +0·94, which was significantly higher at the $P=0{\cdot}01$ level than either of the corresponding values produced by the Thurstone and Likert methods. Eysenck (1954) also claims that: 'This new method has been shown to be more reliable than the other two, and it might be made more reliable still by using an empirical weighting system rather than the fairly mechanical one shown in the text.' This claim is important but appears to rest on the scoring of only one scale. The present writer had hoped to test it further, but encountered certain difficulties in interpreting Eysenck & Crown's description of the technique.

(*iii*) *Some difficulties of interpretation*

First, it obviously will not do simply to multiply the Thurstone scale value of each item by the Likert weights 1–2–3–4–5, which is what Eysenck & Crown might seem to suggest. Suppose one has two items whose scale values are 3 and 10. The 'strongly agree' response of the first item would then be scored 15, and the 'strongly disagree' response 3. Similarly, the second item (with scale value 10) would receive a score of 10 for the 'strongly agree' response and a score of 50 for the 'strongly disagree' response. But in this case the latter item (and all the other items with high scale values) would exercise a disproportionate influence on each respondent's total score, although the direction of scoring is of course arbitrary for an attitude scale, and the scale values might equally well run from 10 to 0 as from 0 to 10. Presumably the correct procedure is to express each scale value as a deviation from the theoretical neutral point of the scale before multiplying it by the Likert weights. This, as we shall see later, is what Castle does.

Furthermore, on turning to the table of weights, it is very difficult to see how Eysenck & Crown's figures can be obtained by even a modified version of the procedure they recommend. In Table 2 all the 'uncertain' replies receive the same score, regardless of the scale value of the item. If, however, the different scale values of the items (or the different deviations from neutrality) are multiplied by Likert weights and are then all divided by a constant, which is what Eysenck & Crown appear to suggest, the 'uncertain' responses ought surely to receive differing Scale Product weights in the same way as all the other responses. Secondly, for each item the five weights add up to 20 (except in one case, which is clearly a misprint). If the scale values or deviations had been multiplied by Likert weights and then divided by a constant, it is hard to see how this could have occurred.

At first sight, an alternative interpretation of Eysenck & Crown's description of the procedure might be that *for each item* the Scale Product weights are divided by a constant, or in other words that one constant is not used for the whole table but a number of different constants, such that for every item the 'uncertain' response is scaled down to a value of 4. But it turns out that this procedure could not produce the set of figures in the table. All it would produce would be a set of slightly magnified Likert weights (1–3–4–5–7) identical for each item.

The more one examines the table in Eysenck & Crown's article, the more difficult it becomes to reconcile it with the account in the text, for the table seems to have been constructed by some method rather more analogous to that used by Likert, Roslow & Murphy (1934). At least it has been shown to possess two features which characterize their method (all 'uncertain' responses scored alike and equal sums of weights for each item), but which are hard to reconcile with Eysenck & Crown's own description.

This analysis of Eysenck & Crown's method is necessarily detailed and critical. It should be added that their account of the Scale Product method is highly condensed and forms only a small part of a long and important paper.

IV. CASTLE'S SCALE PRODUCT METHOD

(*i*) *General characteristics*

The third paper is that of Castle (1953), who uses the method *advocated* by Eysenck & Crown with one or two unimportant variations. Although he makes no claim to originality of technique, his is the only work known to the writer which describes the actual employment of this method. It will therefore be convenient to refer to it as 'Castle's method' to distinguish it from the method exemplified in Eysenck & Crown's table.

Castle's scale products are obtained by first computing the deviation of each item's scale value from the neutral point of the scale, and then multiplying this deviation by the Likert weights; the 'uncertain' responses thus receive different Scale Product weights according to item in the same way as the other responses, and the sum of five weights assigned also varies according to the scale value of the item. One slight peculiarity of his method is that he takes 6·5 as the neutral point of the scale, although his items were originally sorted into 11 piles.

A more serious point of criticism arises from his use of minus weights. The three methods previously discussed which allow for graded responses (i.e. the Likert, Thurstone-weighted, and Eysenck & Crown methods) all use reversed scoring for half of the items in the scale. With the Likert method, for example, it is usual for the items falling at one end of the continuum to be scored 1–2–3–4–5 and those at the other end 5–4–3–2–1. The same principle applies to the Thurstone-weighted and Eysenck & Crown methods, which possess the property that if the scale values of two items are equidistant from and on different sides of the zero point, these items will receive the same weights but in reverse. Thus in all these methods the *range* of weights for the different items is the same for the 'strongly agree' responses as for the 'strongly disagree'. As a consequence of his use of minus weights, this is not true of Castle's scoring, and the defect is aggravated by the large numbers he uses. The result is that the weights he assigns to 'strongly agree' responses range from $+23$ to -21 (for the 5 items tabulated in his paper), and those of the 'strongly disagree' only from $+5$ to -4. This sort of asymmetry seems likely to produce some curious results.

(*ii*) *Reliability*

Castle, following Eysenck & Crown, suggested that his scale was probably more reliable than if it had been scored by the Likert method. But, as we have seen, Castle's method differs in principle from that used by Eysenck & Crown, as inferred from their table of weights. It therefore seemed of interest to determine empirically whether a type of Scale Product scoring approximating to Castle's would produce a higher split-half coefficient of reliability than the ordinary Likert method.

The material used consisted of Part I of a questionnaire on attitudes to education constructed by Oliver (1953). This scale contained 30 items, of which the following are examples: 'Mathematics is valuable for the training it gives in abstract reasoning', and

'Naturalness is more important than good manners in children'. On a sample of 57 Manchester University evening students it had been found to have a split-half reliability (corrected) of +0·92 when scored by the Likert method.

Scale Product weights were calculated on the same principle as Castle's, and the same 57 answer sheets re-scored. The scoring technique differed from Castle's in the following respects: (*a*) the theoretical mid-point of the scale was taken to be not 6·5 but the actual mean scale value of the 30 items, (*b*) reversed scoring was used for half of the items in order to preserve the symmetry of the scale, (*c*) the resulting Scale Product weights were scaled down, as recommended by Eysenck & Crown, both in order to avoid unwieldy numbers and to make them more comparable with Likert weights. The split-half reliability (corrected) was now found to be +0·89. This is lower than the previous value, but a test of significance (Fisher's z test) showed the difference not to be significant at the $P = 0{\cdot}05$ level.

V. COMPARING RELIABILITIES: SOME FURTHER CONSIDERATIONS

To apply a test of significance of this kind to the difference between two reliability coefficients is possibly of doubtful legitimacy when these have been obtained from the same data. The assumption underlying such a test is that the data are independent and uncorrelated, so that both the above comparison and the earlier one between the reliability of the Likert and the Thurstone-weighted methods should be treated with caution. The same is true of Eysenck & Crown's claim for the superior reliability of their Scale Product method. On the other hand, it may well be that the result of applying such a test of significance to coefficients obtained from the same data is to *underestimate* the significance of the difference. In this case it would be only the negative results of the tests of significance reported in the present paper that should be considered suspect. The main point of both comparisons, that neither the Thurstone-weighted nor the Castle technique is more reliable than the Likert, remains in any case unaffected.

Another consideration relevant to the above comparisons of reliability has been mentioned by Vernon (1953), who suggests that the use of both Thurstone-type scaling and item analysis in scale construction tends towards higher reliability. Item analysis does not appear to have been used in the construction either of Eysenck and Crown's or of Castle's scales, but was used in addition to Thurstone scaling in the construction of Oliver's. Thus it is possible that the Scale Product method, in the version either of Eysenck & Crown or Castle, is more reliable than Likert scoring when applied to an ordinary Thurstone scale, but not when applied to one that has been modified by item analysis as used in the Likert method of construction.

I should like to thank Prof. R. A. C. Oliver and Prof. P. E. Vernon for their generous encouragement and helpful criticism during the writing of this note.

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AN INVESTIGATION INTO THE PERCEPTION OF HEIGHT BY RATS

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Three rats were made to jump a barrier to reach food. If the barrier was of uniform height, rats showed no preference for negotiating any particular part of the barrier. If any part of the barrier was lowered by even a small amount, the rats showed preference for this portion. If the barrier was uniformly raised to a height which was not jumped, and then part of it was raised yet higher, the part of the barrier remaining relatively low was jumped successfully. It is concluded that if part of the barrier appears relatively low, the rat may 'decide that it can jump it after all'.

I. Introduction

The experiments described were the outcome of observations made on stray cattle attempting to jump into a plot surrounded by a fence, with a gate of the same height as the fence. They were unsuccessful until the height of the fence was raised 1 ft., the gate being left at the original height. The animals now jumped the gate, which seemed low compared with the rest of the fence. Was this behaviour due to chance factors, or was the animals' success in jumping governed by comparative rather than absolute heights? The following experiments, using rats, suggest that comparative height is an important factor determining an animal's 'estimate' of what it can jump.

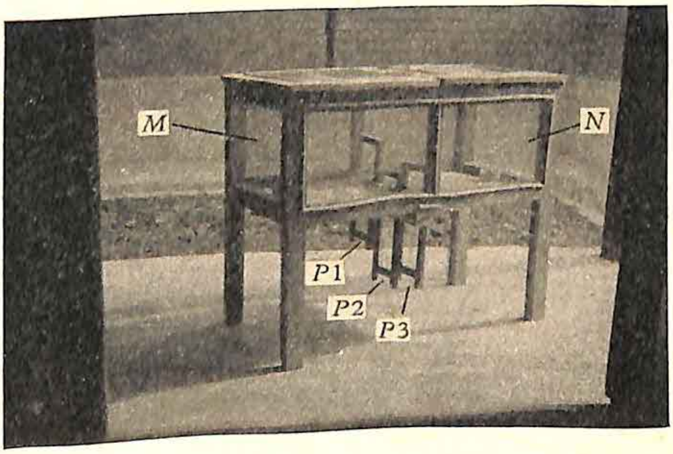

Fig. 1. The cage is closed with the partitions $P1$, $P2$ and $P3$ standing at different heights; M and N are the two halves of the cage.

II. Apparatus

A cage of wood and wire meshing, 4 ft. long, 2 ft. wide and 2 ft. tall, was divided in two by a fence consisting of three equal partitions, made of glass in wooden frames. The partitions could be raised and lowered individually.

III. THE EXPERIMENTS

Healthy common rats of similar size and growth were used. Before experiments they were starved for 24 hr. in a separate cage. The rats were tested individually by placing them in one-half of the experimental cage, food being placed at the mid-point of the remote end of the other half. If the rat jumped the fence, the food was immediately removed and placed in the first half of the cage; and so on. The rats were rested if they showed signs of fatigue or lack of interest. If the rest pause was ineffective, the experiment was suspended for the day, and the rat fed. Five rats started the experiment, but one died and another escaped before the finish.

With all the partitions at the same height, rats tended to jump over the partition nearest the room wall. When the cage was placed so that it was uniformly lit, the rats jumped over any one of the three partitions, apparently at random. As a further control, the cage was moved through 45° between each jump. There were three parts to the experiment. A count was kept throughout of which partitions the rats negotiated. The sequence of presentations was as follows:

A. (*a*) Fifteen jumps, for each rat, with all three partitions (Fig. 1, $P1$, $P2$, $P3$) at the same height of 1·5 in.
  (*b*) Five jumps with $P1$ at 1 in., $P2$ and $P3$ still at 1·5 in.; then similarly with $P2$ and $P3$ lowered in turn, the other two partitions in each case remaining at 1·5 in.
  (*c*) Each partition in turn raised to 2 in., the other two being kept at 1·5 in., five jumps being made by each rat under each condition.

B. (*a*) Fifteen jumps by each rat with $P1$, $P2$, $P3$ all at 6·0 in.
  (*b*) Each partition in turn raised to 6·25 in.; five jumps in each case.
  (*c*) As for (*b*), but raising each partition in turn to 6·5 in.
  (*d*) As for (*b*), but lowering each partition in turn to 5·75 in.
  (*e*) As for (*b*), but lowering each partition in turn to 5·5 in.

C. (*a*) All three partitions were gradually raised together until the rat failed to jump them in three successive attempts. Fatigue was minimized by taking several days over this process. The final height reached varied from rat to rat. This height was taken to be just beyond the 'psychological limit' for the rat.
  (*b*) Starting with all partitions at this height, each portion in turn was lowered by 0·25 in., i.e. to a height which should have been negotiable. If the rat jumped successfully, two more jumps (though not in succession) were allowed for that partition. Similarly for the other two partitions.
  (*c*) As for (*b*), but each partition in turn being lowered by 0·5 in.
  (*d*) As for (*b*), but each partition in turn being raised by 0·25 in., i.e. the barrier was altogether above the previous 'psychological limit'.
  (*e*) As for (*d*), but each partition in turn being raised 0·5 in.

IV. RESULTS

Table 1 represents sum total of successful jumps for the three rats with all the partitions, $P1$, $P2$ and $P3$, at 1·5 in. (A (*a*)); the adjusted partitions lowered at 1 in. and the other two* at 1·5 in. (A (*b*)); and the adjusted partitions raised to 2 in. while the other two remained at 1·5 in. (A (*c*)). In this and the following tables *P* values indicating significance at the 0·05 level or better are shown; in all other cases selection of partitions may be ascribed to chance.

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* It was not possible to determine with certainty as to which of the two partitions standing at the same height was selected, since often when the partitions stood side by side the rats jumped over the line of contact.

Table 2 likewise represents the successful jumps under five varied conditions.

Table 3 represents successful and unsuccessful jumps at the limiting height (l.h.) of the partitions with one adjusted.

Table 1

	Partitions	Jumps	χ^2	d.f.	*P*
A (*a*)	*P*1	16	0·12	2	—
	*P*2	14	—	—	—
	*P*3	15	—	—	—
A (*b*)	Adjusted one	35	38·02	1	<0·001
	The other two*	10	—	—	—
A (*c*)	Adjusted one	7	5·62	1	<0·02
	The other two	38	—	—	—

* Twice as many jumps are expected by chance from the two partitions not adjusted as for the one adjusted.

Table 2

	Partitions	Jumps	χ^2	d.f.	*P*
B (*a*)	*P*1	16	0·13	2	—
	*P*2	15	—	—	—
	*P*3	14	—	—	—
B (*b*)	One raised to 6·25 in.	9	3·02	1	—
	Others at 6 in.	36	—	—	—
B (*c*)	One raised to 6·5 in.	3	13·25	1	0·001
	Others at 6 in.	42	—	—	—
B (*d*)	One lowered to 5·75 in.	28	16·62	1	0·001
	Others at 6 in.	17	—	—	—
B (*e*)	One lowered to 5·5 in.	35	38·02	1	0·001
	Others at 6 in.	10	—	—	—

Table 3

C (*b*)	One lowered by 0·25 in.	15	20·63	1	0·001
	Others at l.h.	2	—	—	—
	Unsuccessful jumps	10	—	—	—
C (*c*)	One lowered by 0·5 in.	21	39·04	1	0·001
	Others at l.h.	0	—	—	—
	Unsuccessful jumps	6	—	—	—
C (*d*)	One raised by 0·25 in.	1	0·03	1	—
	Others at l.h.	4	—	—	—
	Unsuccessful jumps	22	—	—	—
C (*e*)	One raised by 0·5 in.	0	2·16	1	—
	Others at l.h.	7	—	—	—
	Unsuccessful jumps	20	—	—	—

V. DISCUSSION

It can be concluded that rats prefer to jump the lowest part of a barrier, even though the difference in height is as small as between 5·75 and 6·0 in.

Although there is a tendency for the lower partitions to be chosen more often the greater the absolute difference in height of the partitions, this result is nowhere significant.

The results of experiment C show that a height, previously not jumped, may be jumped if it appears low in comparison with some other part of the barrier. This suggests that relative lowness is an important factor in determining a rat's jumping behaviour, a factor which may override a rat's 'assessment of the maximal height it can jump'.

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THE EFFECT OF STIMULUS LIGHT INTENSITY ON REACTION TIME OF SCHIZOPHRENICS

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It is a commonly accepted dictum that schizophrenics show signs of withdrawal under threat of 'punishment'. A possible explanation is that any stimulus producing a larger than optimum degree of excitation will bring about a worsening of response in these patients.

An experimental test of this suggestion was attempted by means of a reaction time (R.T.) study with four different intensities of stimulus light. It was postulated that the higher values of stimulus intensity would produce an effect of increase in reaction time in schizophrenic patients, in contrast to the well-established finding with normal subjects of a non-linear decrease in R.T.

A standard reaction-time procedure was used. The subject was asked to release a Morse key as quickly as possible after the onset of a stimulus light which was presented once every 8 sec. A forewarning buzzer preceded the light at a 3 sec. interval. The timing of buzzer and light was carried out electronically.

Four intensities of stimulus light were used. These were: *A*, 0·26 W., *B*, 0·52 W., *C*, 1·33 W., *D*, 2·25 W. The surfaces surrounding the stimulus light and response key were painted matt black to avoid reflexions.

Twenty presentations of each light intensity were made, order effects being controlled by the use of balanced Latin square design. Three minutes' rest was allowed between each set of twenty presentations.

Six groups of four non-paranoid chronic schizophrenic subjects were used, each group completing a Latin square design.

Two groups of four nurses were used as normal controls. Four of the schizophrenic groups were made up of male and the remaining two of female patients.

The mean age of the male patients was $36{\cdot}0 \pm 5{\cdot}9$ years, of the females $39{\cdot}3 \pm 11{\cdot}2$ years, and of the normal controls $30{\cdot}1 \pm 2{\cdot}1$ years.

In working out the results only responses 6–15 were scored to avoid possible warming up effects and end spurts. The analysis of all twenty responses would, however, have given results similar to those presented here.

The main finding is shown graphically in Fig. 1. In each of the six patient groups after a decrease in reaction time from intensity *A* to intensity *B*, there is an increase in reaction time with intensity *C* and a subsequent decrease with *D*. In the normal group, however, there is a non-significant decrease ($P > 0{\cdot}05$) in reaction time from *A* to *B*, no increase in reaction time from *B* to *C* and an insignificant increase ($P > 0{\cdot}1$) with *D*.

In the schizophrenic subjects the decrease in reaction time from *A* to *B* is significant at the 0·1% level ($t = 4{\cdot}45$). The increase in reaction time from *B* to *C* is significant at the 1% level of confidence ($t = 3{\cdot}26$); the subsequent fall from *C* to *D* is not significant

($t=1{\cdot}63$). No effect of order of presentation was found ($F=0{\cdot}012$). The 3 min. rest period between presentations can thus be considered adequate.

The decrease in R.T. between stimulus intensity A and stimulus intensity B found in twenty-three out of twenty-four of the schizophrenic subjects is in line with that found in normal subjects, but is approximately three times as great as that found with the normal controls tested in this experiment. The increase in reaction time with higher stimulus intensities found in twenty-one out of the twenty-four schizophrenic subjects tested is a finding which is surprising in its regularity and is in some ways akin to the paradoxical effect found in conditioned reflex studies. The decrease in reaction time in schizophrenics from stimulus C to stimulus D, although non-significant, is worthy of further experimentation. Possible explanations of this are: (*a*) the non-continuance of the earlier effect; (*b*) the subject looks away from the brightest stimulus and receives effectively less excitation on the fovea; (*c*) the retina is in a refractory phase after strong stimuli due to the presence of after images.

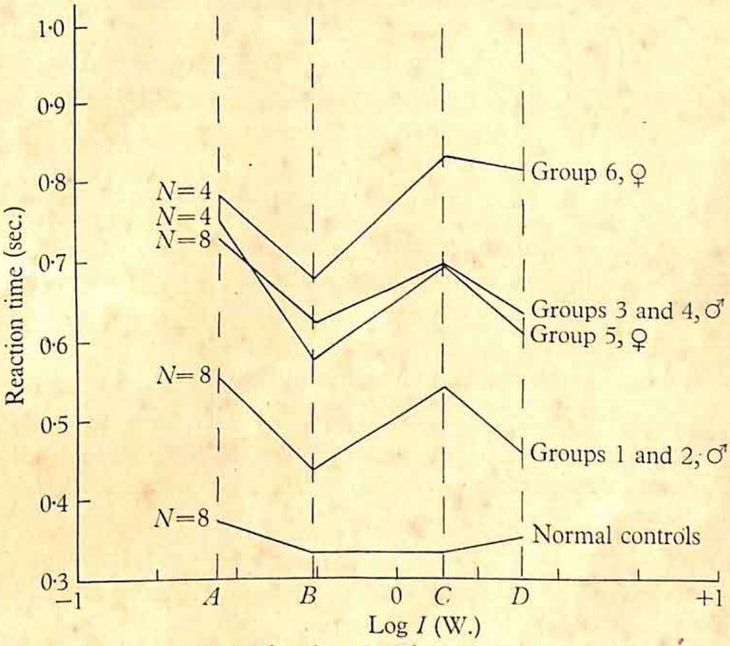

Fig. 1. Reaction times of schizophrenic and normal groups plotted against logarithm of intensity of visual stimulus.

Further experimental work is necessary to test these explanations, and it is hoped to carry out a further experiment with greater control of the stimulus conditions and a larger range of light intensities.

Schizophrenic groups (3, 4 and 5) were at the time of the experiment receiving treatment by chlorpromazine (Largactil); the remainder were receiving neither treatment nor sedatives. It is evident that the presence of the drug does not change the essential pattern of results.

This study was carried out at Springfield Hospital, London, by the kind permission of the Medical Superintendent, Dr H. C. Beccle.

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THE PERFORMANCE OF ONE-YEAR STUDENTS IN THE DEPARTMENT OF EDUCATION, MANCHESTER UNIVERSITY

By F. W. WARBURTON

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This report is concerned with the performance of 95 one-year students in the Department of Education, University of Manchester, in the session 1952–3 and 105 students in the session 1953–4. The students are classified according to sex, and faculty (Arts or Science). Eight predictors of success are considered.

In the School Certificate Examination (all examination boards):

(i) Number of passes, credits and distinctions (total subjects).
(ii) Number of credits and distinctions.
(iii) Number of distinctions.

In the Higher School Certificate Examination (all examination boards):

(iv) Number of scholarship and principal subjects (total subjects).
(v) Number of scholarship and good (principal) subjects.
(vi) Class of University degree (all universities).
(vii) Interview grade.
(viii) Grade of referee's report.

There are two criteria of success:

(i) Theory mark (examination papers plus essays) in the Graduate Certificate of Education.
(ii) Practical teaching mark in the Graduate Certificate of Education.

Product moment correlations were calculated between the predictors and the two criteria of success for the two years combined. (They were very consistent from year to year.)

Predictors	Criteria	
	Theory mark	Practical teaching mark
S.C. total subjects	+0·187**	+0·043
S.C. credits and distinctions	+0·220**	−0·061
S.C. distinctions	+0·168*	−0·019
H.S.C. total subjects	+0·161	−0·011
H.S.C. good (principal) and scholarship subjects	+0·117**	−0·046
University degree (Hons. *v.* Ordinary)	+0·433**	+0·197*
Interview grade	+0·383**	+0·348**
Referee's report	+0·320**	+0·299**

(Significance of correlations: ** highly significant (0·01); * significant (0·05).)

The correlations between the predictors and the criteria of success are rather low, ranging from −0·011 to +0·433. A candidate's academic record at school significantly predicts success only on the theoretical side of the course. The grades given by the referees and by the interviewers are both more predictive than examination results, and are correlated with success in both the theory papers and in practical teaching.

Few significant differences in the variables studied were found between the mean scores of groups of students. Arts students, compared with Science students, have gained more credits and distinctions in the School Certificate Examination, receive higher grades in referees' reports, and score higher in the theoretical papers in the Graduate Certificate of Education. There are no significant differences between the sexes.

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A NOTE ON THE BASAL LEVEL OF SKIN CONDUCTANCE AND BINET I.Q.

By N. O'CONNOR and P. H. VENABLES

Medical Research Council, Unit for Research in Occupational Adaptation,
Institute of Psychiatry, London

A group of imbecile women tested on a resting GSR level, namely micro-mhos conductance and on a dexterity test (GATB tests O and P) as well as on the Binet Scale (Stanford, 1937 Revision) showed a correlation of $-0{\cdot}4$ ($P = 0{\cdot}02$) between Binet I.Q. and resting conductance level. It was inferred that male imbeciles would have a significantly higher level of conductance than an equivalent normal male sample. This was found to be the case ($t = 2{\cdot}75$, $P < 0{\cdot}01$). The results suggest a relationship between mental performance and autonomic reactivity which deserves further investigation.

INTRODUCTION

In connexion with work on learning, motivation and performance (O'Connor & Claridge, 1955), imbecile women were tested on three measures: the Binet (Stanford, 1937 Revision), the finger dexterity tests of the United States Employment Service Battery (called O, assembly, and P, dis-assembly) and a valve voltmeter type of circuit measuring skin conductance. The purpose of these tests was to apply them as controls in eliminating the effects of initial intelligence, dexterity and emotional, or, more correctly, autonomic labilty on subsequent learning or performance.

METHOD AND RESULTS

Forty imbecile women between the ages of 15 and 37 were selected for the larger experiment. On the basis of I.Q.'s and other considerations, such as the existence of epilepsy or the fact that some were receiving drugs, 5 were eliminated. The remaining 35 were of Binet mean I.Q. 36, mean dexterity (O plus P) 24, and mean resting conductance 9·5 micro-mhos. Standard deviations were 4·4, 6·7 and 5·5 respectively. The mean age of the group was 28 years. The scores were intercorrelated with the following results:

	r	P
Binet I.Q. with conductance	−0·40	0·02
Binet I.Q. with finger dexterity	0·48	0·02
Conductance with finger dexterity	−0·21	0·10

Skin conductance was measured under conditions wherein effective temperature did not exceed 66° F. Venables (1955) has suggested that higher temperatures affect basic conductance. Each subject's resting level was assessed as a level reached and sustained after a period of accommodation lasting 20 minutes. These results were accepted as indicative of a possible relationship between resting conductance level and I.Q. in imbecile women. It was inferred that the conductance of imbecile men should be significantly greater than that for normal men. A random selection of 18 male imbeciles of equivalent age and I.Q. was compared with 18 normal men matched for age (Venables, 1955). The means and standard deviations in these samples were:

	Mean	S.D.
Imbecile men	15·36	6·83
Normal men	9·6	2·8

A t test made between the two male groups, one normal and one imbecile, yielded a t of 2·75, equivalent to a P value of 0·01. In this case resting conductance was measured after 4 minutes for both groups.

DISCUSSION

Such a relationship as the one reported has not been found in the literature. A finding which may bear some similarity to this is found in Sleeper & Jellinek's (1936) work on polyuric and non-polyuric schizophrenics, in so far as they believed that their two groups differed in sympathetic reactivity and at the same time in I.Q. There are many possible physiological and biochemical explanations of this cortico-visceral interaction which might be offered, but which the authors are not qualified to evaluate.

The authors wish to acknowledge the considerable help afforded by Dr J. F. MacMahon, Physician Superintendent of the Manor Hospital, Epsom, England, who kindly gave every facility for the work, and that of his staff who made the subjects available.

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PUBLICATIONS RECENTLY RECEIVED

Essays on the Patho-Physiology of the Higher Nervous Activity. By A. G. IVANOV-SMOLENSKY. Moscow: Foreign Languages Publishing House. 1954. Pp. 348. 7*s*. 6*d*.

On but few occasions during recent years has it been possible for outsiders to have other than a very fleeting glimpse of developments in medicine, physiology and psychology within the Soviet Union. Reports from those few who have visited Russian laboratories, and papers read by Soviet scientists at international congresses, have suggested that Pavlovian theory and method are playing central roles in these developments. Therefore, it is of great interest that an English translation of this series of essays has been published. The author was one of Pavlov's colleagues, and the essays were written as a tribute on the occasion of the centenary of Pavlov's birth. The book was awarded a Stalin Prize in 1949, an indication that it accurately presented Russian views in the biological fields with which it is concerned.

There are three main essays, each concerned with a different aspect of the evidence upon which Pavlov's theory of 'the higher nervous activity' is based. The main features of the theory are familiar and are based upon observations employing 'the method of conditional reflexes'. The first two essays discuss research on the 'pathological changes' in conditioned behaviour associated with experimental lesions of the cerebral hemispheres, with exposure to 'difficult tasks' ('experimental neurosis'), with artificially induced disturbances of the internal milieu of the organism (e.g. the endocrine system), with experimental intoxication (e.g. 'morphine intoxication'), and with certain infections (e.g. 'pasteurellosis'). During the course of this research '...it proved possible to create in the laboratory not only experimental (though roughly simplified) models of neurotic syndromes, but also models of different (in particular, toxicogenic) psychotic states ('experimental psychoses'). Experimental therapy of these pathological states also receives considerable attention, with emphasis on retraining, on medication (e.g. bromides, caffeine, phenamine), and on sleep (natural and induced). Because Pavlov assumed '...that the fundamental laws of nervous activity must be identical in man and in animals, both in normal and in pathological states...' it is logical that he and his colleagues should have generalized theory and experimental results from their infra-human 'models' to man. The third essay is devoted to these generalizations as they have been applied in the psychiatric and in the neurological clinic.

The essays provide a very favourable testimonial for comprehensive research planning on a grand scale, in this instance covering some fifty years and still in progress. Throughout the essays, as throughout this long period of research, Pavlovian theory serves the dual role of integrating the results of numerous individual projects and of generating new ideas for further investigation. Even those who are critical of certain aspects of the theory would agree that it has served these two roles well.

A book of this kind is likely to do more than merely satisfy a reader's curiosity regarding trends in research and theory construction. It may create a desire to do something with the information it imparts—to examine certain research studies in greater detail, to try out in the laboratory or the clinic some of the methods and ideas described, to see whether or not the research data can be fitted into other theoretical frameworks. To do any of these he will need more detailed information than any single book covering such a wide range of research could be expected to provide. In 240 pages of text are summarized reports to which references are made in 50 pages of bibliography, and this does not include all of the work done in Pavlov's laboratories. The author has described new ideas, new methods and new results, but to compress so much material into such a limited space has meant restricting the amount of detail which could be included. He has referred to research papers containing the details, but here the non-Russian reader encounters language difficulties which prevent him from exploring matters of particular interest or examining conclusions in greater depth.

Language difficulties may also account for the fact that developments in 'patho-physiology' outside Russia receive but little attention in the essays. One of the author's objectives was '...to expound...the basic achievements of this young branch of science'. To attain this objective would require an integration of the considerable amounts of research done by workers outside as well as inside Russia.

The main contribution of the essays to the 'patho-physiology of the higher nervous activity' is '...to give an outline of its development in the Pavlov school (including its ramifications) up to our time'. 'The Pavlov school' is taken very literally to refer to Russian scientists who have worked within the frame of conventional Pavlov theory. It does not include workers from other countries who made contributions under Pavlov's guidance, but who later altered certain aspects of his point of view. To place the essays in an adequate perspective as regards the entire field of 'patho-physiology' it is important to keep these limits of the author's objectives in mind. This tribute to Pavlov is not concerned with his widespread influence on thinking and research beyond his own laboratories and clinics.

It is also important for the reader to remember that the School's definition of 'psychology' differs from that generally accepted in this country. '...Pavlov invariably and in every possible way avoided interpreting the resulting disturbances from a psychological point of view: he always strove to study the disturbances of the neurophysiological functions, the derangements in the dynamics of the higher nervous processes....' Such 'dynamics' are generally inferred from observations of behaviour employing 'the method of conditional reflexes'. This general approach also characterizes the theory construction of contemporary psychologists, e.g. Tolman and Hull. Psychologists generally will feel quite at home in reading of Pavlov's attempts '...to superimpose a psychopathological pattern on the neurophysiological canvas'.

The important role which 'the Pavlov school' plays in contemporary Russian psychology, physiology and medicine is reflected in the essays, and provides added incentive to examine their contents carefully. If they are made generally available they may serve as an important step in integrating Russian information in the field of 'patho-physiology' with comparable information from other countries. R. W. RUSSELL

Entwicklungspsychologie des zeichnerischen Gestaltens. By G. MÜHLE. Munich: Barth. 1955. Pp. viii+165. DM. 24.

The term 'gestalten', verb-noun of gestalt, has no English equivalent. It means a selecting, forming, structuring activity which is part of the process of producing a work of art. But it is also used in the wider sense to describe the process of shaping and structuring experiences (Erlebnisse) in any medium. Mühle takes as the subject of his book, first, the problem of gestalten as such and, secondly, gestalten in drawing as a contribution to a general developmental theory of gestalten. This book is divided into three sections, namely, the problem of pictorial ability, the process of gestaltung, and fundamentals of a theory of gestaltung in drawing.

There is very little information on the material on which the author bases his investigations or on the methods by which he arrives at his conclusions. He claims to have traced the development of free drawing in childhood up to about the tenth year, but there is no statement as to the number of subjects or the conditions under which the drawings were produced. The term 'free drawing' is not defined but, from the illustrations, it looks as if a large proportion of the drawings were executed on instruction, under class-room conditions, with a given subject (e.g. our village), or even a model such as a bowl of fruit.

The gestalt approach forms the theoretical basis of the investigation described as the 'genetic psychology of wholes and structures' of Krueger 'completed and systematically built up' by Wellek. The author claims that he is not dogmatic as to his theoretical approach, yet he makes very definite reservations by excluding literature in the fields: of comparative psychology of gestaltung, 'because fundamentals for such a task are as yet not sufficiently clarified'; of typology, 'because a true typology of drawing gestaltung promises to be fruitful at later age groups only'; and of depth-psychology, 'because the far reaching separation of conscious and unconscious, so important in adulthood, would only be found as the result of development and not in comparable form in the child'.

The method of presentation is discursive, the reader having to follow the tortuous course of critical argument with some 200 books and articles in reference to the author's statements. These references are quoted in full and, along with numerous cross-references, continually interrupt the flow of the book.

Generally speaking, Mühle finds that the development of gestaltung leads from the undifferentiated to the differentiated. A pattern of gestaltung has to be acquired and established at each stage of development, 'up to the highest planes of artistic production', before an individual 'loosening' towards expression can be attempted. This continuous patterning process is, however, superimposed by a theory of values. 'Gestaltung—even that of children—is distinguished by a definite striving towards the realization of values.'

Although the author tries to keep a fair balance in discussing the different aspects of gestaltung, he sets up a hierarchy that leads from the exclusive dominance of 'tactual-motor and emotional factors' in early childhood to the 'true' gestalten which (following Klages) 'is deeply penetrated by a noetic and volitional attitude and integrated in consciousness in a peculiar way'. Accordingly, 'true' development tends towards the representational, and it is the increasing dominance of intellectual factors which determines the degree of 'trueness' of gestaltung. The author is, however, well aware of the influence of

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Apart from the general theoretical weakness, there is a wealth of valuable and interesting detail. The chapter on 'Abstraction and concretion' is well-reasoned and convincing and should go a long way to clear misconceptions regarding the 'abstract' art of children.

MARTHA VIDOR

Nebraska Symposium on Motivation. 1955. By MARSHALL R. JONES (ed.). Lincoln, Nebraska: Univ. of Nebraska Press. 1955. Distributed in United Kingdom by Bailey Bros. and Swinfen, Ltd., London. Pp. x+274. 24*s*.

This is the report of the third annual Symposium on 'Current Theory and Research in Motivation' at the University of Nebraska. Each of the six symposiasts contributes a paper, which is followed by comments from other participants.

In the first paper, Maslow urges psychologists to give up their detachment and to accept an obligation to offer advice on vital human problems. The importance of these problems seems to him to justify recourse to 'inexact and unreliable researches' where others are lacking. The rest of his paper consists of a demonstration that 'inexact and unreliable researches' can lead different psychologists to very different conclusions, so that the net gain from them is not obvious. There are plenty of ungracious people about who remain to be convinced that psychology should not be done away with. They will claim that the only thing they are ever told by a psychologist, that they would not have heard from somebody else if psychology had never existed, is how wrong other psychologists are. One must confess, without denying that Maslow makes some good and telling points, that it would be embarrassing to have one of those killjoys looking over one's shoulder, just when one happens to be reading this paper of his.

There is no danger, on the other hand, in allowing the killjoy access to the appetizing stuff served up by Olds. He refurbishes some of the essential concepts, sums up recent work on the reticular formation and the limbic system, including his own epoch-making experiments which might mean the discovery of a 'reward centre' in the brain, and offers a theory which serves to tie existing data together and guide future research. In its choice of ingredients, its mode of preparation and its judicious blending of cautious assertation and piquant speculation, his contribution is exemplary.

The remaining papers lie between these two poles in manner and in matter. Young sums up some important findings on the relations between palatability of food and motivation in rats. He claims that S-R formulations could not be reconciled with these findings, but he does not make clear which S-R formulations he is thinking of and precisely what reasoning brings him to this conclusion. Peak and Rotter offer theories of human motivation which reflect some dissatisfaction with current conceptions derived in large measure from animal experiments. Their theories are eclectic in origin but seem much of the time to be rephrasing common notions in technical language—e.g. that what one does depends on what one would like and what one thinks will lead to it. Such theories may allay some impatience with the slow process of working one's way up from simple behaviour in simple organisms, since they seem to be directly concerned with everyday human activities. But their fruitfulness, no less than that of animal experiments, can only be proved in the not too near future. A promising intermediate position is represented by McClelland, who builds on theories derived from animal psychology but is not afraid to modify them radically and to use clinical-psychology techniques to test them. The researches he reports appear, however, to have more practical and methodological than fundamental theoretical importance.

D. E. BERLYNE

Psychoanalysis and Ethics. By L. S. FEUER. Oxford: Blackwell Scientific Publications. 1955. Pp. vi+134. 30*s*.

This essay is divided into three parts. In Part One the author is primarily concerned to propound the thesis that psychoanalysis gives us a method of 'verifying the ultimate value' of a society. To assert that a value is 'ultimate' is, in effect, to assert that 'there is a corresponding unconditioned and irreducible drive in the human organism'. We 'verify' this value by (I take it) applying the psychodynamic generalizations of psychoanalytic theory to discover whether a given value is 'expressive of a primal drive of the organism or whether it is anxiety-induced'. In this way ethics becomes a branch of social science, and the 'psychoanalytic criticism of values is thus the primary work of the ethical philosopher'.

Part Two examines and rejects Freud's pessimistic view of civilization as being necessarily incapable of bringing happiness to men. In the brief Part Three the author raises three interesting questions. Is social science committed to the limiting self-doubt of cultural relativity? The answer is 'No', because 'there is a common ideal for humanity founded on laws of psychology common to all human beings'. Is illusion, contrary to Freud's assumption, a necessary safeguard for the happiness of man? The author confesses that he is uncertain how to answer this question. Finally, he argues that a liberal civilization begins when the age of ideology is over—when we have progressed beyond it.

I think it is largely because I sympathize with the author's own 'ultimate values' that I find this book intensely irritating. The main thesis of Part One is so sketchily stated and incompletely worked out that it will convince few who are not convinced already. The difficulties and limitations of applying psychoanalysis for 'the verification of ultimate values' are simply not acknowledged. I am forced to say that the author gives me the impression of not beginning to understand the point of Stevenson's *linguistic* analysis of moral discourse. His criticism of trend in Part Two is apt, but it is not new. In the sixteen pages of Part Three, the author does not succeed in doing more than tantalize us. What we require from him is a more rigorous and fuller treatment of the topics he has raised. B. A. FARRELL

Sigmund Freud. Life and work. Vol. II. By ERNEST JONES. London: The Hogarth Press. 1955. Pp. xviii + 534. 30*s*.

This volume covers the years 1901 to 1919—'Freud's years of maturity', as Jones describes them. During these years there appeared three contributions of major importance for the development of Freud's general theory, viz. *The Psychopathology of Everyday Life, Three Essays on the Theory of Sexuality* and the essay 'On Narcissism'; and it was during this period that Freud laid the foundations of the psychoanalytical theory of the psychoses in his study of the famous case of Schreber (a paranoid schizophrenic) and his essay on 'Mourning and Melancholia'. Further, the appearance of *Totem and Taboo* and the study of Leonardo da Vinci during the same period registered Freud's application of psychoanalytical theory to the wider fields of religion and art.

These years were of special importance for the psychoanalytical movement, with the fortunes of which those of Freud himself were so intimately bound; for it was during these years that the movement attained international status, and that psychoanalysis achieved, if not general acceptance, at least general recognition. Such recognition was only achieved in face of an intensity of opposition which the contemporary world has largely forgotten, but of which the reader of Jones's account will be vividly reminded. This was, however, a period of inward no less than of outward storm for the psychoanalytical movement; for it witnessed the recurrent crises which eventually led to the respective defections of Adler, Stekel and Jung. The inner history of these events, with the clash of personalities which characterized them, is described by Jones in illuminating detail; and the reader is left in no doubt as to the real issues involved. It was a critical phase in the history of the psychoanalytical movement, but one out of which eventuated the consolidation of the movement and the establishment of the characteristic psychoanalytical outlook as now understood. The dissensions which preceded the final outcome inevitably imposed a heavy strain upon Freud; and it was to relieve this strain upon him that, at the instigation of Jones, an inner Committee of six trusted analysts (including Ferenczi, Abraham and Jones himself) was established as an Old Guard round Freud to supervise the affairs of the movement and to free Freud's energies for concentration upon his researches. It may surprise even those who are familiar with the outstanding intellectual penetration, forcefulness and practical acumen of Jones to learn what a central role he played in moulding the psychoanalytical movement into the form which it assumes to-day.

As in the previous volume of the biography, the personal life of Freud receives fully as much attention as the development of his theoretical views; and, as a psychoanalyst describing the life of the founder of psychoanalysis, Jones conceives it as his duty to apply psychoanalytical insight to the interpretation of Freud's personality and the conduct of his life. The exacting nature of this self-imposed task may well be imagined, since Freud was not only an intimate friend of the author, but also, understandably, a hero in his eyes; but the task is discharged with characteristic objectivity. It would be a mistake, of course, to expect a degree of objectivity of which human nature is incapable, or even that degree of objectivity which is expected of a case-history; and there can be no doubt that, if Jones had been writing a case-history instead of a biography, the analysis of the psychoneurotic aspect of Freud's personality would have been carried considerably further. Here it may be noted that what many regard as a strain of pessimism in Freud's outlook is interpreted by Jones as an unusual degree of realism; but it is difficult to ignore the extent of Freud's preoccupation with death as brought out in the biography—a feature which may well have some bearing on his controversial concept of 'the death instincts'.

Whilst the reader cannot help being grateful to Jones for his vivid account of the day-to-day life of a great man, it is difficult to avoid feeling that the account is sometimes burdened with excessive detail, especially where the minutiae of Freud's journeys and holidays are concerned. A case in point is the statement that on the second day of his first visit to Rome Freud 'rode in a *fiacre*, at two lire an hour, from three to seven'. The visit itself was, however, notable in that Freud described it as 'the high-point of my life'. Rome's extraordinary fascination for Freud provides an intriguing subject for speculation, especially in view of the accompanying inhibition which for many years deterred him from approaching nearer to the city, then Trasimeno, where Hannibal, with whom Freud appears to have made a powerful identification, was likewise halted. Whilst Jones regards the appeal of Rome to Freud as due at a conscious level to Freud's interest in archaeology, he interprets it at an unconscious level in terms of the symbolic significance of the Mother-City; and it is to the influence of this symbolism that he attributes the taboo implied in Freud's inhibition over entering Rome no less than the city's magical appeal. In Jones's opinion also the unconscious, no less than the conscious, appeal of Rome to Freud was strictly that of ancient Rome, as against the medieval Rome of the Holy Catholic Church. Indeed Jones finds it quite incredible that Christian Rome could have had any appeal whatever to one who spoke openly of 'the lie of salvation'. If Jones is right, it would thus appear that Freud simply rejected Christian Rome, and that his ambivalent attitude to the city was exclusively concerned with pagan Rome. This is perhaps the least convincing of all the biographer's interpretations. Thus it would appear significant in the light of psychoanalytical principles that (and here we may be grateful for some minutiae) the first place in Rome which Freud visited was, not the Coliseum, but St Peter's. Further, 'the lie of salvation' is sufficiently forceful and emotional a phrase to suggest an overcompensation—especially coming from the lips of one who devoted his life to devising something akin to a means of salvation for tortured minds. And, in addition, if Freud could be ambivalent to the pagan Rome which consciously fascinated him, it is difficult to see why he should not be equally ambivalent to the Christian Rome which consciously repelled him. His identification with Hannibal may also have a wider significance than at first appears. After all, Hannibal approached Rome as a would-be destroyer; and, if the author of *The Future of an Illusion* approached Rome as the would-be destroyer of Mother-Church, it is not difficult, in the light of psychoanalytical principles, to explain the inhibition which for so long denied to Freud entry into the Holy City.

It is interesting to note that Freud's life, as evidenced by Jones's biography, was characterized by a remarkable dualism. Not only do we find in Freud one who conceived human life as governed by the interplay of the dual forces of libido and aggression, but also one who called salvation a lie and devised a means of effecting something very like salvation, a neurologist who, whilst never abandoning the hope of ultimate physiological explanations, found it impossible to understand human behaviour in other than psychological terms, a moral puritan who described life in terms of sexuality, a philosophical stoic who was also an intellectual hedonist, a loyal Jew who hated Judaism, and a Viennese who disliked Vienna, but continued to live there until he was driven out of it. Withal, there emerges from Jones's biography the picture of one of the world's geniuses, an intellectual giant, a tireless investigator, an inveterate searcher after the truth, a man of indomitable courage and strength of purpose, a character of irreproachable integrity, a personality at once lovable and forceful, an understanding physician, a loyal friend, a faithful husband and a devoted father.

W. RONALD D. FAIRBAIRN

Mysterium Conjunctionis—Part I. By C. G. Jung. Zurich: Rascher Verlag. 1955. Pp. xv+284. 22·90 Sw. fr.

The subtitle of this latest of Jung's works, which the author actually calls his 'last', introduces it as: 'An investigation on the separation and synthesis of psychic opposites in Alchemy'. The chapters contained in this first volume are concerned with the symbolism of polarity and union.

In the last two decades Jung has been increasingly attracted by the symbolic structure of alchemical concepts and aims. While in '*Psychology and Alchemy*', which first appeared eleven years ago, he described the inner relatedness between these two fields of human endeavour, the present book is devoted to a detailed investigation of alchemical tests, which the author sets out to compare with endopsychic events as they may, for instance, appear in the analysis of dreams and active imaginations. In this task he succeeds most admirably. The reader is bound to become engrossed in the magic of the master magician, as Jung himself is so obviously fascinated by his medieval predecessors in spirit. Their aim was not, as

Jung points out, to make ordinary gold but rather to produce a lapis philosophorum, the essence of wisdom, which can only come into being through a process of separation and analysis on the one hand and of unification and growth on the other. The drama of human development is seen in the mirror image of the alchemical opus. The opposing elements in man's soul are expressed in the symbolic language of medieval mysticism. Jung foresees the possibility that later generations may feel his work to be as 'metaphoric' and symbolic as we who live to-day regard the alchemical mysticism. Characteristically he adds: 'I do not imagine that the psychological interpretation of a mysterium necessarily constitutes the *last* word.'

The book is very clear in structure. Never does the author lose sight of his theme in spite of the awe-inspiring abundance of the material presented. Ample use is made of footnotes (some of which incidentally contain pearls of great price) and the text remains lucid and simple. This is no mean achievement in view of the very involved and obscure language of a good many texts used. Yet it would not be an easy book to read for the uninitiated, that is, those who have not acquired their 'transference' to Jung before. The visionary character of Jung's work and his intellectual honesty, however, cannot fail to impress his followers and critics alike.

Dr von Franz, the co-editor, must be congratulated on a masterly use of the literature belonging to this vast subject.

W. P. KRAEMER

Psychanalyse et Conception Spiritualiste de l'Homme. By JOSEPH NUTTIN. Publications Universitaires de Louvain. 1955. Pp. 367. Paper, 150 Belgian francs; cloth, 180 Belgian francs.

This is yet another book about Freudian psychoanalysis by a man who has not been analysed. As such books go it is quite good. Prof. Nuttin has read very carefully the works of Freud and of many of his followers, and, having high standards of intellectual honesty and accuracy, admits many of the facts they have discovered, and describes their theories clearly. In places, however, he wastes time on hair-splitting. He discusses at length two slightly different theories of sublimation, then misses the vital human problem—that many people have been taught that it is their Christian duty to love their fellow human beings, and also that sex is more or less bad and dangerous, and yet have enough introspective ability to know, at least dimly, that all love is closely related to sex. He can be very unsubtle. He writes that those who deal with psychological conflicts should have a great love of the normal, yet fails to realise that this great love is to be found only in neurotics.

The least satisfactory part of the book is Prof. Nuttin's own theory of normal psychology. This is because he tries to describe the adult without taking account of his previous development, and at the same time assumes that the psychology of children is simple and closely related to physiological mechanisms. This leaves adult psychology hanging in the air, separated from its real roots in the complex inner lives of small children, who have scarcely begun to feel the bodily and the mental as separate.

Some of his criticisms of Freud and his followers so often imply that normality is a kind of unhappy superstructure on the life of natural instinct.

Prof. Nuttin is perhaps at his best in discussing problems on the borderland of psychoanalysis and religion, such as the Christian acceptance of suffering. If he would only widen his experience by undergoing analysis himself—perhaps a Jungian one would suit his particular cast of mind better than a Freudian—he might achieve something really outstanding in this field.

J. DARROCH

Mental Health and Infant Development. Vols. I and II. By KENNETH SODDY (ed.). London: Routledge and Kegan Paul Ltd. 1955. Vol. I (Papers and Discussions), Pp. xix+308. 25*s*. Vol. II. (Case-histories), Pp. v+289. 25*s*.

These volumes were compiled from material presented and discussed at a Residential International Seminar held at Bishop Otter College, Chichester, in 1952 and sponsored by The World Federation of Mental Health.

International Conferences have become popular battle grounds in recent years, and of Mental Health Conferences there have been not a few: this particular gathering, however, was essentially an experiment in multi-professional education in mental health, and as such it appears to have been remarkable in a number of ways.

It was highly ambitious—as indeed are most Conferences in principle and programme—but, in reading these volumes, it becomes obvious that its ambition was in large measure achieved, viz. to provide for participants an opportunity for experiencing mutual understanding and learning, and to widen their horizons so that they might gain a greater insight into the personal and cultural problems of the people among whom they would be working.

To have, living at close quarters for three weeks, seventy-five people (of whom twenty-four were Faculty Members from Great Britain, France and America, and fifty-one participants from sixteen European countries and fourteen other parts of the world) with a variety of professional and cultural backgrounds, discussing highly controversial matters, might well have produced only a chaos of tensions and difficulties with little apparent constructive progress in any direction. So carefully, however, had the Seminar been planned, and so thoroughly had an astounding amount of preliminary work been carried out over a period of two or more years previously, that the Conference itself might be considered an outstanding example of international co-operation; while the two volumes to which the Seminar gave birth may be regarded as a monument to what may be achieved, even in the short space of three weeks, by mutual learning.

The general theme of the Seminar was that of the phenomena of family life in the first two years of a child. With such a wide range of subject-matter and such diversity of professional interests, it was inevitable that some grouping for discussion should occur. Contrary to more usual practice, the grouping arrangements were highly structured with a heterogeneity of race, language and profession. Such a grouping might well lead to relatively fruitless discussion due to an insufficient fund of common information about matters to be considered. Here again, the Chichester Seminar set an excellent example of good organization by distributing beforehand, to all participants, the individual case-histories which were to form the material for teaching and discussion.

While the centres of interest of many mental health conferences seem to lie in the abnormal rather than the normal, during the first of the three weeks at Chichester emphasis was studiedly that of normal child development, with thence a natural shift to the family as a dynamic whole vitally essential to both mental and physical well-being, though manifesting its influence somewhat differently in different cultures which, in themselves, are continuously and from generation to generation undergoing modification.

In view of the particularly rapid changes in patterns of living which have taken place within the past ten to twenty years, the Conference gave special attention to both the theoretical and practical aspects of the need for flexibility in mental health education rather than the comparative rigidity of training by didactic and traditional teaching. The false sense of security which the latter may induce can at times act as a serious hindrance to adaptability. Such a situation, incidentally, was exemplified within the Seminar itself by the cyclic pattern of some participants' own attitudes towards the discussion methods adopted.

In Vol. I are presented the lectures given to the full Conference, together with summaries of discussions arising from them and from the reports of the five special working groups; in Vol. II are presented carefully chosen representative samples of the case-histories which formed the basic material. Throughout Vol. I, frequent reference is made to material in the histories given in Vol. II; to derive the maximum value from these records, the two volumes need to be read in close conjunction. Both, incidentally, are eminently 'readable', even deceptively so in many ways for, with the commendable absence of unnecessary jargon and consequent clarity of expression, it would be easy to be misled into too rapid a reading of material so closely integrated that it presents too much substance for immediate digestion without a great deal of subsequent rumination.

The case-histories in Vol. II were intended ultimately to be made available for teaching purposes, and for this they will be invaluable, not only now, but in the future; they are, in fact, a most valuable library of documents for future research in a number of fields.

One notable point which emerged from the transactions of this Seminar was that, in a field of inquiry limited to the first two years of life, there exists a wealth of material with appeal to a variety of interests other than those ordinarily, and at first thought, associated with infant care; and that research into attendant problems becomes more fruitful and misses fewer important issues if all aspects provide contributions to our knowledge.

Whatever we do or fail to do, human society continues to exist; what lies in our power is only to help or hinder certain changes. Quotations from two papers by the anthropologist Margaret Mead seem to put the position clearly.

'However far back we go in human history we always find the family. There is no human society in the world without it and no human society has succeeded in abolishing it permanently' (Vol. I, p. 163); and,

'...we are doing the same thing to human relations as we did in the conquest of nature, plants and elements. ...One of the dangers we face...is to think of the natural scientist as someone who bends nature to his will, whereas he only proceeds by understanding the laws of nature. Once he has discovered some new law of nature, it is possible for him to work with it, to develop compensatory devices, but nevertheless always to work with it. In our own work we try to do the same thing; we try to discover something about the basic biological laws of human development, so that we can work with them.' (Vol. I, p. 184).

Despite the invaluable help given by so many groups and individuals, both in the preparation of material beforehand and in the subsequent organization and compilation, the final task of editing such a mass of data must have been heavy. Dr Kenneth Soddy has, however, made an admirable job of it, and we are indebted to him for making the work of the Seminar, which he directed, available to those who did not have the privilege of participation.

M. I. DUNSDON

A Psychological Study of a City's Cerebral Palsied Children. By ELLA B. FLOYER. London: The British Council for the Welfare of Spastics. 1955. Pp. 88. 3*s*. 6*d*.

This inexpensive little booklet is one of the best introductions to the psychological aspects of Cerebral Palsy that has appeared. The British Council for the Welfare of Spastics is to be congratulated for sponsoring its publication.

The findings have the advantage of being based on personal investigations of all the cases in the school age-range known to Liverpool Education Authority in March 1950. They therefore give a complete picture of the problem as it appears to such an authority. There are some grounds for believing that it may not give such a complete picture of the medical and social problem. An incidence of 1·3 per 1000 school-children, such as is reported, falls well short of the figure of 2 to 2·5 cases per 1000 suggested by Ingram on the basis of his Edinburgh survey. It is likely, however, that the cases omitted are, for the most part, too dull or too bright to cause educational problems.

The quantitative findings, upon which the educational recommendations for the group are based, are set out clearly. The source and numbers of the samples, from which percentages are derived, are obvious, for which a reader has reason to be grateful, in view of some deficiencies in these respects in other works in this field.

The chapter on 'Intelligence of Cerebral Palsied Children' is a very sane statement of well-established findings, and their educational implications. The vexed question of the 'unfairness' of applying tests standardized on normal children to this handicapped group is carefully and cogently considered.

The comments on 'Behavioural Deviations from the Normal' are necessarily less precisely stated, since less precisely observed, than those on intellectual assessment, but the characteristics described form such an important part of the problem that they well merit their place. They are classified as distractibility, tension, lack of drive, disinhibition, fluctuation in performance, fits, perseveration, emotional instability, emotional and social immaturity. Some tidying up of categories will no doubt result from further research, but, in the meantime, it is valuable that the possible relationship of such deviations to brain injury should be publicized. The frequent failures of such children to progress educationally at the rate their psychometric level suggests, might then be regarded as occasion for diagnosis rather than exhortation. One might, however, query the assumption that the occurrence of convulsive seizures necessarily indicates emotional instability. One might, too, have wished that space could have been found for a chapter on psychological disabilities which were neither 'general intellectual' nor those classified as 'behavioural' deviations, rather than the tantalizingly brief comments on, for example, visuo-spatial disability tucked away in the chapter on 'Some General Practical Considerations' and the Appendix.

But that is to ask for a bigger book than this modest pamphlet, which serves its purpose admirably as it is.

J. A. L. NAUGHTON

Adolescent Development. (Second edition.) By ELIZABETH B. HURLOCK. London: McGraw-Hill. 1955. Pp. xiii+590. 45*s*.

This is a new, up-to-date edition of one of the best-known text-books on adolescence. Recent research is fully reported in all the different areas, and there is special emphasis on the treatment of social class as a determinant of behaviour, juvenile delinquency, new trends in courtship behaviour, social acceptability, and the effects of physical changes on attitudes and behaviour. There is also a new chapter on family relationships.

Like all Hurlock's books, this one is characterized by its very factual tone. Quite obviously a thorough reading of the literature on adolescent development has led to a setting down of the major empirical results. There is very little theory in this book and no attempt has been made to unify in any way the data reported, or to study them from the point of view of any kind of theoretical concept. To some readers this may appear an advantage; it certainly acts as an antidote to the anecdotal theorizing so frequently indulged in by many clinical psychologists in this area.

Results are reported mainly in the form of verbal summaries; Hurlock does not reprint original tables. This, oddly enough, is justified on the grounds first that all studies cannot be reported in detail, and that it would be difficult to select some and reject others! 'Furthermore, there is a growing belief on the part of many educators that few students read the tables presented in their text-books. It would seem advisable, therefore, to eliminate them and use the space to report the important findings of the studies.' This is a debatable point. What is, perhaps, less debatable is the undesirability of the habit Hurlock shares with other American text-book writers of illustrating perfectly obvious points with photographs and diagrams which add nothing to the discussion. It is difficult to see how the discussion of family influences (page 488) is helped by a half-page photograph of a father, looking as if he had indigestion, looking quizzically at his son, who appears to be in the throes of a tooth-ache. Similarly, the contribution on page 401 of a half-page picture of a boy and a girl standing under a sign apparently advertizing a pawn-shop to a discussion of dating and courtship, in spite of its realistic reference to a money-lender, is only tangential. The whole-page cartoon on page 377 illustrates 'The seven stages in boy-girl relationships', but it is difficult to see how this will help the psychological sophistication of the reader. Devices of this kind will tend to repel rather than attract students in this country, but it would be a pity if instructors allowed these aberrations to lead them to overlook the solid contribution made by the text.

H. J. EYSENCK

Teacher, Pupil, and Task. Edited by O. A. OESER. London: Tavistock Publications. 1955. Pp. xiii + 195. 18*s.* net.

The subtitles of this book indicate clearly enough its scope and intention. They are: 'Elements of Social Psychology applied to Education' and 'A Practical Manual for Teachers'. It is addressed to the teacher, being the contents of a course for Australian teachers, and, as such, has comparatively little interest for the psychologist. The theme is 'adjustment', and though the bulk of the book is devoted to social aspects of education, there are chapters on examinations and vocational guidance which stretch the term 'social' rather far. For the teacher, the book undoubtedly will have value, but it does not wholly escape the danger inherent in such expositions, that of implying that the findings of psychology are much simpler and more direct than they really are. A companion volume, on the theoretical foundations, is promised; this should be of more interest to the readers of this *Journal*.

JAMES MAXWELL

Psychology of Industrial Behaviour. By H. C. SMITH. London: McGraw-Hill. 1955. Pp. 477. 45*s.*

This is a text-book with a message. As a *text*, it uses all the artifice and sugaring of the pill that we are accustomed to in American books nowadays. It is, in fact, very clearly set out, and refers to quite a wide range of empirical work, some of it unfamiliar. The scope is wider than that of the recent Viteles book, concentrating on the social aspects of industrial psychology, but including chapters on Tests, Human Engineering, and Training. The quality is rather uneven, the chapters on Job Satisfaction, Job Anxiety, and Union-Management relations being the most interesting. One or two sections caused the reviewer to raise his eyebrows—for example, there is a confusion of the influence of wage incentives on *production* and on *costs* (p. 33); the chapter on productivity measurement does not mention time-study; group bonus schemes are enthusiastically praised (p. 200f.), but no supporting evidence is provided; and it is maintained that the span of control should be as large as possible (p. 242f.), in the interests of decentralization. The author does dispel some of the perennial myths of popular industrial psychology—for instance, that happy workers work harder, and that supervisory training courses increase production—but he still believes that wage incentives are ineffective in comparison with 'human problems', that communications need to be improved, and that strikes are largely due to unconscious motives.

The *message* of this book is that management and unions should co-operate to reach their joint goals, and that the obstacles to reaching these are largely human problems which can be solved by applying psychological knowledge. Insufficient attention is paid to genuine conflicts between these goals and to conflicts arising from the application of psychological knowledge: for example, evidence is cited to show that men work hardest under a medium degree of anxiety (p. 101 f.), but also that anxiety is a major source of low job satisfaction (p. 99 f.). A secondary message is that rigorous scientific methods should be used to solve the problems, though the author is not quite so aware of this when asserting his generalizations.

MICHAEL ARGYLE

Anxiety and Stress, an Interdisciplinary Study of a Life Situation. By HAROLD BASOWITZ, HAROLD PERSKY, SHELDON J. KORCHIN and ROY R. GRINKER. From the Institute for Psychosomatic and Psychiatric Research and Training of the Michael Reese Hospital, Chicago. New York: McGraw-Hill. 1955. Pp. xv + 320. Price not stated.

The difficulty about reviewing publications of this kind is that on each occasion one finds oneself saying the same thing over and over again. The opening paragraph of the authors' own preface was presumably intended to be self-explanatory: 'With the background of clinical and laboratory studies on men in war and on patients with neurotic anxiety, we undertook the present studies of healthy young American men under the stress of a real life situation—paratroop training. In the faith that somatic, psychological, affective, and behavioral processes are transactional, performances during various stressful occasions and through time are not viewed as independent entities. Rather, they are parts of the total psychosomatic-social field in constant interdependent participation and change.' Considering its brevity, it is a fair statement of the authors' psychological orientation. All too soon, however, it leads to the hope of the 'possible contribution of this conception and approach to the further development of the behavioural sciences'. What is this expectation based on? Seven groups of men, ranging from eight to thirty individuals, were repeatedly subjected during the course of military training in parachute 'jumping' to various combinations, as suggested by convention, of the following procedures: interview, questionnaire (including self-rating), projection test, Bender-Gestalt test (the cards being projected and the exposure time controlled), a tachistoscopic version of Landolt's rings, memory for digits, serial subtraction, and a number of biochemical tests (e.g. for any excess of hippuric acid in urine, for deficiency in eosinophil cells and glutathione in the blood, etc.). In spite of the labour that must have gone into the preparation of the chapter on the Intercorrelations among Experimental Procedures, no new or otherwise important psycho-physiological correlation has been discovered. Indeed, most of the biochemical indices tested had to be eventually 'interpreted to mean that these situations are not sufficiently stressful'. The main finding of psychological interest is referred to as the 'stress end phenomenon', to mean an increase in anxiety as assessed, after the completion of training. This presumably was the immediate occasion for the authors' decision to write a monograph of this length, apparently in order to 'propose an explanation that under stress individuals must exercise a maximum of psychological control in order to cope successfully with the many threats inherent in the training situation. With the training ended, such control is loosened, permitting the free expression of anxiety hitherto contained.' The authors are careful not to miss the opportunity to note that 'this is not unlike the instances reported during World War II of the development of severe anxiety among troops rotated from the front lines or combat air missions *after* they had reached rest areas removed from hazard'. So far as the reviewer's reading goes, such tendencies were not unknown to medical observers during World War I. Admittedly, in view of the fact that their study was carried out at the time of the Korean war, the authors do recognize the possibility of other interpretations. They feel, however, that the 'end phenomenon reflects a basic and theoretically important aspect of stress behaviour'—though this is as far as they are prepared to advance their argument at all. Not unexpectedly, therefore, the monograph has to end with these phrases: 'In summary, the implications of the present research have been far-reaching to those who have experienced its trials and tribulations.... The gain from the work has been less in increasing our knowledge of the details of psychosomatic problems and more in learning the complexities of psychosomatic organization and integration.... The problem of anxiety is still *non liquet*, but perhaps we know a little more why this is so.' Or, perhaps, we do not.

J. SZAFRAN

Anthropologische Psychologie. By OTTO TUMLIRZ. Basel: Ernst Reinhardt Verlag. 1955. Pp. 469. 22 Swiss francs.

This book is the second edition, slightly changed from the original one, which appeared in 1939. It is very typical of German psychology and anthropology of the present day. There are practically no references to Anglo-Saxon work; McDougall and Watson, and the early Spearman, are almost the only non-German authors named. The discussion, altogether, is oriented very much in accordance with McDougall's instinctive psychology, and everywhere exudes the atmosphere of the *Schreibtisch*. Its only interest lies in the contrast between its completely non-experimental approach and the much more factual type of work which would be included in a book of such a title in this country.

H. J. EYSENCK

Michelangelo: a Study in the Nature of Art. By ADRIAN STOKES. London: Tavistock Publications Ltd. 1955. Pp. 154. 25*s.* net.

This is a short book in which the author, who is a writer on aesthetic subjects, uses Michelangelo's work as a basis for exploring the use of psychoanalytic concepts in understanding the formal qualities of art. The book consists of three short sections dealing respectively with the statement of the theory, together with a brief account of Michelangelo's life, then a discussion of the visual works, followed by comments on the poems. Each section is supplemented by notes and there are appendices on iconography and on the Medici Chapel.

This reviewer must confess to finding the book obscure and of doubtful value. Mr Stokes's thinking and his style both seem to suffer rather than profit by his somewhat uncritical excursions into Freudian literature and language. In addition, though the author writes with respect for a great artist and an interestingly complex person, Michelangelo is not well served by such poor reproductions of his works.

JOAN WYNN REEVES

OTHER PUBLICATIONS RECEIVED

Leadership and Isolation: a Study of Personality in Inter-personal Relations. By Helen H. Jennings. London: Longmans, Green. 1950. Pp. xvii+349. 30*s.* (Second edition.)

Personality and Life: a Practical Guide to Personality Improvement. By Louis P. Thorpe. London: Longmans, Green. 1941. Pp. xiii+266. 25*s.*

The Psychology of Abnormal People. By John J. B. Morgan and George D. Lovell. London: Longmans, Green. 1948. Pp. xi+673. 35*s.*

Psychological Testing. By James L. Mursell. London: Longmans, Green. 1949. Pp. xvi+488. 32*s.* 6*d.* (Second edition.)

Expression of the Emotions in Man and Animal. By Charles Darwin with an introduction by Margaret Mead. New York: Philosophical Library. 1955. Pp. xi+372 and added illustrations. $6.00.

Task Variability and the Level of Aspiration. By J. P. Sutcliffe. *Australian Journal of Psychol. Monogr. Suppl.* No. 2. Melbourne: University Press. Pp. 85. 12*s.* 6*d.*

Psychotherapy and Counseling. By L. K. Frank, Rollo May, and 46 others. *Annals of New York Academy of Sciences,* Vol. 63, Art. 3. New York: Academy of Sciences. 1955. Pp. 113. $3.50.

Pestalozzis 'Lebenskrise' und seine Auffassung vom Menschen. By Jaakko Toivio. *Annals of the Finnish Academy of Sciences,* Series B, Vol. 91. Helsinki. 1955. Pp. 347. 1.200 mk.

Child Psychology. By Edna Balint. Cambridge University Press. 1956. Pp. 24. 2*s.* 6*d.* (A National Book League reader's guide.)

The Police and the Mental Health of Children. A study published with the assistance of UNESCO. Pp. 54. 2*s.*

Education and the Moral Basis of Citizenship: a Lecture. By Eric James. London: Heinemann. 1955. Pp. 24. 1*s.* 6*d.*

Man in his Relationships. By H. Westmann (ed.). London: Routledge and Kegan Paul. 1955. Pp. viii+140. 14*s.*

On the Nature of Man. By D. D. Runes. New York: Philosophical Library. 1956. Pp. 105. $3.00.

Some Aspects of Research in Educational Psychology in the USSR. By Brian Simon. London: Bulletin of Soc. for Cultural Relations with USSR, Vol. 2, No. 4. 1955. Pp. 20. 1*s.* 6*d.*

Charakter-Kunde von A bis Z. By Erich A. Oppenheim. Bern: Verlag Hans Huber. 1955. Pp. 297. No price given.

Astrologie. By Gustav Lebzeltern. Vienna: Franz Deuticke. 1955. Pp. 87. No price given.

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THE EFFECTS OF PROPAGANDA ON RECALL, RECOGNITION AND BEHAVIOUR

I. THE RELATIONSHIP BETWEEN THE DIFFERENT MEASURES OF PROPAGANDA EFFECTIVENESS

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Two experiments are described in which the effects of propaganda were assessed by the standard forms of recall and recognition tests as well as by any change in overt behaviour following the propaganda. The first, a field experiment, using road-safety propaganda presented in a Children's Cinema Club, showed that there was no positive relationship between the effects of the propaganda as shown by the tests of recall or recognition on the one hand and behaviour on the roads on the other. A second experiment was conducted in the laboratory where an attempt was made to influence a subject's choice of fast or slow speed on a mechanical task by means of incidental propaganda. The 'effect' of the propaganda varied according to whether its influence was assessed by its attention value, its intelligibility, by how much could be described or recognized, or by its capacity to influence behaviour. Some subjects had only attended to individual items of the display and, although they were able to recall or recognize these, they were not able to understand the implication of the propaganda and were not influenced by it. Others understood and recalled the propaganda but were not influenced by it. Finally, there were those subjects who understood the propaganda and were influenced by it but who could not remember it. Thus it seems that the information which has been learned from propaganda can become apparent in two distinct forms: (a) in recall or recognition, and (b) in subsequent behaviour. The reasons why these different effects were more marked in some subjects than in others will be examined in the second part of this paper.

I. INTRODUCTION

It is the aim of most modern propaganda to influence the action of individuals or groups in some specific way. Experimental studies which have been concerned with assessing its effects have, however, concentrated not on measuring resultant action, but on estimating its influence on some intermediary response such as measuring how well the propaganda can be recalled or recognized or whether it has effected a so-called 'attitude' change as measured by responses given on a questionnaire. It has usually been assumed that propaganda—or in fact many forms of learning—will affect the action of the individual in proportion to the extent to which its content can be recalled or recognized and that a statement made on an attitude questionnaire will predict an individual's behaviour. The present study is concerned with assessing to what extent, and how, the various measures of so-called effectiveness are related—or whether, in fact, they are necessarily related at all.

Experiments on the effect of propaganda have been conducted mainly in two fields of study: first, in relation to Commercial Advertising and Market Research, and, secondly, in the study of attitude change. In the former we find that the effect of an advertisement on buying behaviour has been assessed by anything from the observation of eye movements, tests of recall and recognition, and paired associate tests (Link, 1934; Poffenberger, 1932) to a more recent technique based on the psychogalvanic reflex (Eckstrand & Gilliland, 1948). In 1932, courses were being organized, as a result of these tests, on the psychology of advertising. The methods which were developed then have varied little over the subsequent years (Gallup, 1950). Attention value is considered to be of overriding importance. Unlike much other experimental work, advertising research has been strictly utilitarian and a systematic study of the theory and methodology has lagged behind. Attempts to validate the tests by relating the results to actual behaviour* are desultory and not encouraging (Jenkins, 1938; Poffenberger, 1932; Welch, 1941). For example, an attempt to assess the extent to which the results of orthodox readership surveys were dependable showed that only 47 % of the subjects actually mentioned a periodical which they usually read (Longstaff & Laybourn, 1949).

The measurement of people's beliefs and attitudes is also assumed to be an adequate prediction of social behaviour. It is in this field of attitude research that the vast majority of experiments on the effects of propaganda have been conducted. That 'attitude' changes (as measured by attitude scales and questionnaires) can be brought about by various means has been amply demonstrated (Chen, 1933; Gardiner, 1935; McCreary, 1953; Smith, 1943). But basic to the whole problem is the extent to which these data are capable of predicting with any certainty the course of action that will follow the establishment of any given attitude. Verbalizations elicited in one social context do not always enable us to predict with accuracy non-verbal behaviour in another. Attitude tests are, in fact, eliciting verbal responses in relation to mental pictures of a situation. The general assumption has been made, however, that these tests are true indicators of both verbal and non-verbal behaviour of people towards actual objects and situations rather than being intellectual or socially conditional beliefs or opinions. Such attempts as have been made to validate attitude tests in the light of subsequent behaviour have shown their prediction to be poor. For example, a questionnaire was sent to hotels and restaurants in the United States by La Piere who had previously visited them in company with two Chinese students. The questionnaire asked 'Would you be willing to accept members of the Chinese race as guests in your establishment?' All but one of the replies was negative. As a matter of behavioural fact, he and his friends had been rejected six months before by only one proprietor (La Piere, 1934). A further difficulty in assessing the relationship between attitudinal change and behavioural change is that, in the traditional pre-test post-test procedure, no account is taken of the effect which the pre-test may have on raising the subject's susceptibility to propaganda. A pre-tested audience is no longer typical of audiences in general. Jarrett & Sherriffs (1953) claim novelty value for their experiment in so far as the traditional pre-test design is rejected in favour of matched groups with a post-test only.

It would seem clear from the foregoing that tests of recall and recognition and measurements of 'attitude' do not lend confidence to the belief that they are capable of accurately predicting behavioural response. Conversely, it has been apparent in fields unrelated to our present study that a behavioural response does not always imply that an equivalent response can be made verbally. For example, the process of inductive concept formation is often accompanied by an inability to give the concept a verbal formulation (Hanfmann, 1941). Similarly, it is a commonplace that, in skilled activity, we cannot recall the manner in which our skill is exercised. In the field of abnormal psychology there is a good deal of evidence that, after simple instructions, correct responses are possible without conscious memory of the origin of the association. The hospitalized Korsakov patient will admit no knowledge of his surroundings although he nevertheless behaves as if he were in hospital

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* Throughout this study the term behaviour is used to denote non-verbal action.

(MacCurdy, 1928). Stern reports how a patient suffering from amnesia '...could write shorthand, which he had learnt during the forgotten period, with undiminished skill' (Stern, 1938). There is evidence, too, from the studies of material presented below threshold, that subjects are able to respond to stimuli which they cannot report verbally (Lazarus & McCleary, 1951; Miller, 1939). In none of the experimental studies, however, is there evidence to show the degree to which, or how, these two forms of response are related.

Previous experiments on the effects of verbal and pictorial propaganda have usually been designed in the form

Display of propaganda A.
Recall or recognition of propaganda A.
Display of propaganda B.
Recall or recognition of propaganda B.

The relative effectiveness of the propaganda A and B have then been compared, but the effects of each on action have not been considered. Such experiments follow the tradition of the classical investigations of the learning of verbal material which have been conducted exclusively in the field of memory. What and how a subject learns verbally has been examined by what he remembers. Theories of learning have been built up round the way in which individuals respond to and later report either meaningful or nonsense verbal material. The only studies of experimental psychology which seem to be concerned with the effect of symbols on action are to be found either in the fields of animal and infant psychology (e.g. discrimination experiments) or in a few experiments related to transfer of training in which verbal training is said to assist the learning of a skill (e.g. the effect of mental rehearsal on piano playing investigated by Rubin-Rabson in 1941). Yet whether it is in the social contact of groups or in the use of mass media, the ultimate importance of verbal messages in real life situations is their influence on action. The main attention has been on making learning situations quantifiable. Their application to real life problems has received secondary consideration. It is in an attempt to gain a better understanding of the relationship between the standard criteria of effectiveness and the effect as measured by social behaviour that the present study has been conducted, where experiments have been designed to measure the effectiveness of propaganda on recall, recognition and behaviour.

II. EXPERIMENT 1

(i) *Experimental procedure*

A field experiment was designed to measure the effect of propaganda on children's behaviour on road crossings. This experiment is reported more fully elsewhere, i.e. Belbin (1950*a*, 1953). It was conducted on members of Children's Saturday-morning Cinema Clubs, i.e. on a 'closed community'. Propaganda in relation to the use of pedestrian crossings was presented within one club on three occasions during a year as follows: propaganda A: four film slides; propaganda B: talk by a police sergeant; and propaganda C: a cartoon slide. In another district, an eight-minute children's coloured film was shown and was followed by a slide specific to the local traffic situation (propaganda D).

The following measures of effectiveness were recorded:

(*a*) *Observations of the children's behaviour* on the crossings as they left the cinemas were made throughout the year, i.e. both before and after the propaganda. Observers were posted at each of the crossings and behaviour was recorded as follows:

	Correct use	*Incorrect use*
Pedestrian crossings (3 observers on each)	Those who used 50% or more of the crossing	Those who crossed without using the crossing or who used less than 50% of it
Police-controlled crossings (3 observers on each)	Those who approached the police to be escorted across	Those who went across at uncontrolled points
Traffic-light crossings (6 observers on each)	Crossing when the lights were green	Crossing when the lights were red

The observers were not informed as to when the propaganda would be presented.

Individual behaviour, as well as total numbers, was recorded after propaganda A and B in order to compare it with the amount of propaganda which was remembered or recognized. For this purpose children were handed cards, one colour to those who had crossed correctly and another colour to those who had crossed incorrectly. A slide had been shown, during the club performance, telling the children that these cards would be given to them outside the cinema for a lucky number competition. They were asked to fill in their name, age and school. (Free seats were given to a number of the card holders on the following week.) It was thus possible for the experimenter to follow up individual children to their schools to give them tests of recall and recognition while, at the same time, knowing, by the colour of their card, whether each child had used the crossing correctly or not.

(*b*) *Recall of the propaganda.* Those children between the ages of 7 and 14 years who had received cards at the crossings were visited at their schools on the Mondays following propaganda A and B by teams of research workers and students. They were asked to say what they remembered of various items in the film programme, one of the items being the road safety propaganda.

(*c*) *Recognition of the propaganda.* After propaganda A a recognition test was given to each child who had received a card. After propaganda C, all the children who had visited the Cinema Club on the previous Saturday were given a recognition test on the Monday morning by members of the staff in their schools.

(*d*) *Attention to propaganda.* Photographs of the children were taken during the presentation of propaganda and during the film programmes and were synchronized with sound recordings of the amount of noise made by the children.

The experimental design may thus be represented as follows:

Pre-test (observation of behaviour).

Propaganda.

Post-test (observation of behaviour, measurement of attention, recall and recognition).

(ii) *Results*

(*a*) *Behaviour on crossings.* On the whole, no interesting changes were observed as a result of the propaganda. There was no improvement at all in percentage correct behaviour during the 3 weeks after propaganda A and only on one police-controlled crossing was there any measurable improvement after propaganda B. This latter appeared reasonable, since this particular propaganda was a talk directed towards the use of police-controlled crossings. However, an even greater improvement 3 weeks after the propaganda suggested that the cause for improvement lay in the identity of the police who were on duty and not in the propaganda. Those who were well-known to the children were easily able to induce satisfactory behaviour on the roads. After propaganda C there was a slight decrease in the percentage incorrect behaviour of the girls but there was no similar decrease for boys. When the percentage correct use of crossings before and after propaganda D is ranked for both boys and girls (Table 1) it can be seen that there was no apparent improvement in behaviour as a result of propaganda. Week 5 does in fact show an expected average value for both boys and girls.

Table 1. *Rank order of percentage correct use of crossings before and after propaganda D*

	Crossing 1		Crossing 2		Crossing 3	
Week	Boys	Girls	Boys	Girls	Boys	Girls
1	4	2	2	1	2	2
2	3	1	4	2	1	5
3	2	5	1	3	4	4
4	5	4	3	4	5	3
			Propaganda			
5	1	3	5	5	3	1

(b) *Attention to the propaganda.* Photographs taken of the children during the performance and during the presentation of propaganda offered no criterion of attention. From these photographs there seemed to be less concentrated attention on the propaganda than on the films, especially during the talk of propaganda B. Synchronization of sound recordings with the photography, however, revealed that the apparent attention to the films as judged by the photographs was misleading, since the children were making far too much noise for any of them to have heard what was being said. The children appeared to follow a story visually but were highly selective in attending to the sound track. It is of interest to note that a few children who were identified in the photographs as looking directly at the propaganda slide could neither recall the content of the slide nor recognize it when shown a copy.

(c) *Recall and recognition of the propaganda.* 80 % of the children who were interviewed were able to describe at least one of the slides of propaganda A. 48 % of them described two or more slides. 90 % correctly identified one or more of the slides in the recognition test.

After propaganda B, the children were asked to say anything they could about the talk given at the club. Seventy-four of the ninety-six children who were questioned could recall at least one point in the talk which was related to crossing the road at the appropriate place.

After propaganda C (a cartoon slide of a chicken crossing the road by way of a pedestrian crossing with the inscription 'Use the proper Crossing Boys'), a copy of the slide with the crossing and wording removed was sent to seventeen schools. The children who had been to the club were asked if they remembered having seen this picture and if so why it was shown to them and what was written on it. Of the 536 children who had attended the cinema, 160 did not recognize having seen the slide. Of the 376 who said they did recognize it, 153 knew that it had something to do with pedestrian crossings. Only one child knew the exact five words on the slide. Thus, if a standard form of recognition test had been used as a criterion of effectiveness of the propaganda, it would have been 'effective' to 376 children. When a test of recall was used the propaganda was 'effective' to 153 children.

(*d*) *Individual behaviour in relation to recognition or recall.* An attempt was made to discover whether those who best recognized or recalled the propaganda were, in fact, those who had used the crossings correctly. The high standard of recognition of the slides in propaganda A made it difficult to relate any particular level of recognition to any particular group. Table 2, however, which shows the number of children who expressed no doubts in their identifications, is of some interest. It will be seen that those who crossed correctly and those who crossed incorrectly made about the same number of errors in the recognition test.

Table 2. *The relationship between individual behaviour and recognition after propaganda A*

(Children who expressed no doubts in their identifications.)

	Boys		Girls	
	No. of subjects	Identification errors in recognition test of 9 pictures	No. of subjects	Identification errors in recognition test of 9 pictures
Used the crossings correctly	21	32	17	18
Used the crossings incorrectly	24	37	7	9

From Table 3 it can also be seen that there was no uniform relationship between individual behaviour and recall after propaganda A.

Table 3. *The relationship between individual behaviour and recall after propaganda A*

(Number of children who could recall the slides.)

	Slide 1	Slide 2	Slide 3	Slide 4
Used the crossings correctly:				
Boys (26)	2	15	13	6
Girls (23)	2	9	9	9
Used the crossings incorrectly:				
Boys (32)	4	10	18	6
Girls (11)	3	4	7	7

Similarly, there was no uniform relationship between recall of the talk of propaganda B and behaviour on the crossings after it. Every child who had used the crossing incorrectly was able to recall at least one of the items relating to use of crossings in the talk.

(*e*) *The effects of age or intelligence.* An attempt was made to investigate the influence of two other variables—age and intelligence—on the results so far obtained. First, the question arose as to whether recall, recognition or behaviour varied within the different age or intelligence groups. Secondly, since there seemed to be no change in behaviour large enough to be measurable in the conditions under examination (in spite of the high recognition scores), it was of interest to note whether the children who attended the club formed a selected group, i.e. whether they had particularly low or high I.Q.'s in relation to the rest of the population.

An analysis of the results, in relation to the ages of the children concerned, suggested that, while behaviour on the crossings varied very slightly with age (older children and the 9-year-olds being the worst offenders), there was no general continuous change with age in the recognition or recall scores.

Intelligence quotients were not available from the majority of schools in the area. I.Q.'s (Spearman Group Test) from all members over the age of 11 were obtained from one school, however, and were compared with individual success in the tests after propaganda C. There was no interesting relationship between I.Q. and ability to recall the wording or the purpose of the propaganda.

The I.Q.'s of all the members in the school were analysed in order to investigate whether the Cinema Club members were less intelligent than non-members. In fact, the members and ex-members were found to be slightly more intelligent than non-members (median for members and ex-members = 94·5 and for non-members = 90). Those who gave up membership of the club before reaching the age limit seemed to be the least intelligent of the groups.

(iii) *Discussion*

It was clear from this experiment that there was no uniform positive relationship between recall or recognition of the propaganda on the one hand and behaviour on the other. Individual success in the tests was no criterion of behaviour on the roads. These findings are in conflict with the usually accepted belief that propaganda will affect behaviour in proportion to the extent to which it can be remembered or recognized. It would seem that the standard methods of measuring 'effectiveness' present, on the whole, a vastly exaggerated picture of the powers of road safety propaganda. Some reservation is necessary, however, before arriving at any definite conclusions. Certain variables in the field situation may have masked the ultimate effectiveness of the propaganda. For example, it may have made its mark on the children by heightening their consciousness to potential accident situations without changing their behaviour. Secondly, it has not yet been possible to indicate the underlying causes of the apparent lack of relationship between the influence of propaganda on behaviour on the one hand and the extent to which it can be remembered or recognized on the other. It was thought that the nature and extent of such a relationship could best be studied in the more controlled conditions which are possible in laboratory situations. Further experiments were designed accordingly. The first of these is described below.

III. Experiment 2

(i) *Introduction*

The previous experiment demonstrated that the ultimate success of propaganda cannot be predicted from tests of recall and recognition. If we are to examine more closely the degree to which and under what conditions a relationship exists between the different forms of response, it will be necessary to study the effects, in greater detail, on individuals rather than on groups.

One of the difficulties of studying the effects of propaganda is to find a suitable task to which propaganda can be applied without imposing conditions of artificiality. Indeed, the value of any experiment in social psychology can be gauged, to a great extent, by the

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degree to which artificiality is reduced. Successful laboratory experiments in social psychology have partly met this problem by disguising the real object of the study. This was a technique that seemed to recommend itself especially to the study of propaganda. Accordingly, a laboratory experiment was designed to investigate the influence of propaganda on performance on a skilled task and was conducted in such a way that the learning of the propaganda would take place *incidentally* in order that the general character of the response might approximate as far as possible to the real life situation.

In this experiment, 'propaganda' was left next to the instructions for a task, and its influence on the subjects' subsequent performance was measured in relation to the time they spent attending to it, how much they understood it and how well they could describe or recognize it.|

(ii) *Experimental procedure*

(*a*) *Design of experiment.* Fifty-eight subjects (naval ratings, university students and 14-year-old schoolchildren) were given a mechanical task. Their aim was to score as high as possible in a given time, choosing one of two speeds. The printed instructions pointed out the relative advantages of using each speed. Experience on the task soon revealed that higher scores could be made at the slow speed. However, a propaganda display placed next to the instructions suggested that better results were gained by choosing the fast speed. The experiment was conducted under two conditions: Group A: propaganda was displayed while the subjects were learning the task; Group B: propaganda was displayed when the subjects had learned the task and had thus formed their own opinion of which speed produced the highest scores.

(*b*) *The task.* A 'triple-tester' was used. The speed of the rotating drum on which the 'dot targets' appeared could be adjusted by the experimenter. Both speeds were demonstrated and the subjects were able to choose before each of five half-minute runs which one they preferred. The scores were not visible to the subject. After the fifth run they were asked to choose a speed for a longer run but were not actually given the task. Group B returned the following day and, after revising the instructions (beside which the propaganda was now displayed), they were told to choose the fast or slow speed for a longer run. Thus, their choice on the second day (which might be affected by the propaganda) could be measured against the choice they had recorded on the previous day.

(*c*) *The propaganda.* The implication of the propaganda was that the highest scores on the task had been made by those who had chosen the fast speed. A coloured histogram chart showed the scores of previous subjects who had chosen the fast speed in relation to those who had chosen the slow.

(*d*) *Methods of measuring propaganda 'effectiveness'. Attention*: Subjects were allowed to read the instructions for as long as they wished. From the other side of the room, the experimenter was able to measure, with a silent stop-watch, how much of this time had been devoted to attending to the adjacent propaganda. *Behaviour*: The choice of fast or slow speed for each round was noted. For group B, the speed which they preferred for the longer-run, both before and after seeing the propaganda, was recorded. *Recall of propaganda*: Subjects were asked to draw and to describe as much as they could of the diagram which had been next to the instructions. *Understanding of propaganda*: In order to assess whether subjects had understood the implication of the propaganda as a whole, a form of reconstruction test was given. Subjects had to match four sets of five statements each and then to fit a blank card (which represented the propaganda) into one of these sets. The test, which is described fully elsewhere (Belbin, 1953), attempted to measure understanding of the propaganda without involving verbalization. *Recognition of the propaganda*: Each subject was given a recognition test and asked to identify the 'diagram next to the instructions'.

(iii) *Results*

Attention to propaganda. There was a striking contrast between the two groups both in the numbers who attended to the propaganda and in the amount of time they spent in attending to it.

In group A, seven subjects attended for

$$20,\ 10,\ 9\tfrac{1}{2},\ 7\tfrac{1}{2},\ 5,\ 2,\ 1\tfrac{1}{2}=55\tfrac{1}{2}\text{ sec. (mean}=7\cdot 9\text{ sec.)}$$

In group B, twenty subjects attended for

$$59,\ 50,\ 47,\ 40,\ 38\cdot 2,\ 29,\ 28\cdot 5,\ 28\cdot 1,\ 21,\ 20,\ 15,\ 12\cdot 5,\ 5\cdot 5,\ 5,\ 4\cdot 5,\ 4,\ 3\cdot 5,\ 2,\ 1,\ 0\cdot 5=414\cdot 3\text{ sec.}$$
$$\text{(mean}=20\cdot 7\text{ sec.)}$$

Of these twenty-seven subjects who attended, thirteen understood it, fourteen did not. The amount of time spent in attending varied from 2 to 59 sec. for those who did understand it, and from 0·5 to 50 sec. for those who did not.

Recall. Of the forty-one subjects who could describe the propaganda in full or part detail, thirteen understood it and twenty-eight did not. Four subjects, with recall scores of 5, 5, 4 and 3 out of a possible 6, gave a converse interpretation of it, i.e. that it was better to choose slow than fast.

On the other hand, some subjects who had understood the propaganda were not able to describe it at all. One subject, for example, who had obviously understood the propaganda was only able to recall 'a graph with something red on it'. He was quite convinced that the implication of the graph was that a fast speed was better than slow, although he was quite unable to describe how he gained that impression. The recall scores of those who did understand the implication of the propaganda varied from 1·25 to 6 points.

Recognition. Table 4 shows the number of subjects who were successful in the recognition test compared with those who attended to the propaganda. It can be seen that nine subjects, who had attended to the propaganda, said they did not recognize it. As all these nine subjects had been able to recall at least some of the propaganda, recognition may have been adversely affected by the interpolated recall (Belbin, 1950*b*). However, nineteen of the subjects, who had not 'attended' to the propaganda at all, said they recognized it. These nineteen could only have seen the propaganda peripherally whilst engaged on the task of reading the instructions. It would have been impossible for them to have understood its content or to have drawn any conclusions from it since the detail was too great

Table 4. *Comparison of those who attended to the propaganda with those who recognized it*

	No. who attended	No. who did not attend
Correct identification	18	19
Incorrect identification	9	12

to be covered by a single span of apprehension. Of the thirty-seven subjects who correctly identified the propaganda in the recognition test, only eight understood it.

Behavioural change. Of the eighteen subjects who originally chose slow for the long run, four changed to fast after seeing the propaganda. None of the eleven subjects who originally chose fast for the long run changed the speed after seeing the propaganda. One of the four who changed to fast had not understood and could not, therefore, have been influenced by the propaganda. It is interesting to note that the other three (all of whom admitted that their change of speed had been made because of the propaganda) were, in fact, reverting to their original choice of speed for the first five runs before experience on the task had changed their decision. The propaganda was in fact serving to reinforce an original decision.

Effect of propaganda under the two different conditions. In the learning situation (group A): seven subjects attended to the propaganda, of whom two understood it, of whom two chose the fast rate. When the task had been learned (group B): twenty subjects attended to the propaganda, of whom eleven understood it, of whom five chose the fast rate, of whom three had been influenced by the propaganda (one had originally chosen fast, one had not understood the propaganda).

Table 5 shows the test results of the subjects whose behaviour was influenced by the propaganda.

Table 5. *Test results of subjects whose behaviour was influenced by the propaganda*

	Attention time in seconds	Items recalled (total 6)	Whether propaganda was correctly identified in recognition test
Group A	9·5	4·0	No
	20·0	6·0	Yes
Group B	12·5	5·5	No
	28·1	6·0	No
	38·2	3·5	No

It can be seen from this table that neither duration of attention nor number of points recalled were any guide to the ultimate effect of propaganda on behaviour. The results of the recognition test suggest that non-recognition was a better guide to effectiveness! If a recognition test had been used as the criterion of effectiveness thirty-seven subjects would have been successful.

IV. Discussion

It is clear from these results that the effect of the propaganda varied according to whether the subjects had had previous experience on the task and according to whether its influence was assessed by its attention value, its intelligibility, or by its capacity to influence behaviour.

Some subjects had only attended to individual items of the display and, although they were able to recall or recognize these, they were not able to understand the propaganda and were not influenced by it. Other subjects understood and recalled or recognized the propaganda but were not influenced by it. Finally, there were those subjects who understood the propaganda and were influenced by it but who could not remember it. Thus it seems that the information which had been learned from the propaganda material could become apparent in two distinct forms: (*a*) in recall or recognition, or (*b*) in subsequent behaviour. It was not clear, however, what caused effect (*a*) or (*b*) to be more marked in some subjects than in others.

One of the main disadvantages of using a recognition test was its inability to predict *what* a subject had actually perceived in the display. In many cases, recognition seemed to be based on memory for visual form rather than for meaningful content. Vernon (1945) has shown how individuals sometimes clearly visualize graphical material without translating the images into the information the graph was intended to convey. It is thus hardly surprising that responses in recognition tests and those in tests of 'behaviour' are so little related.

Similar factors could not explain, however, why some subjects were not influenced by the propaganda although they could recall and understand it. Nor, from a study of the literature, is there a comprehensive theory upon which all our results could be founded. Bergson 1913, and later McDougall (1923), distinguished between two ways in which the past '...appears to be stored up', postulating motor mechanisms making use of the past in action, and 'pure' memory being the ability to call up the image of an experience. The term 'use of the past in action', while at first sight appearing to resemble the response of our subjects who used the information, was actually envisaged by Bergson as comparable to habit formation expressed, for example, in verbal repetition habits where the memorization was exclusively dependent upon specific motor mechanisms. It is thus hardly comparable to our groups who could not verbalize but who were able to extract the information from the propaganda for use in another task. A different theoretical assumption has been made by Katona (1940) and Szekely (1950*a*, *b*), who demonstrated how information could either be recalled or used in relation to problem solving or mechanical tasks respectively. They suggest that the difference which exists is not solely one of memory function but is due to different learning processes being involved at the outset. The extent to which disparity between the tests of recall and tests of actual behaviour may be interpreted in terms such as these cannot at present be estimated. It will first be necessary to consider in more detail the conditions which determine a response to propaganda. These will be examined in the second part of this paper.

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It can be seen from this table that neither duration of attention nor number of points recalled were any guide to the ultimate effect of propaganda on behaviour. The results of the recognition test suggest that non-recognition was a better guide to effectiveness! If a recognition test had been used as the criterion of effectiveness thirty-seven subjects would have been successful.

IV. Discussion

It is clear from these results that the effect of the propaganda varied according to whether the subjects had had previous experience on the task and according to whether its influence was assessed by its attention value, its intelligibility, or by its capacity to influence behaviour.

Some subjects had only attended to individual items of the display and, although they were able to recall or recognize these, they were not able to understand the propaganda and were not influenced by it. Other subjects understood and recalled or recognized the propaganda but were not influenced by it. Finally, there were those subjects who understood the propaganda and were influenced by it but who could not remember it. Thus it seems that the information which had been learned from the propaganda material could become apparent in two distinct forms: (*a*) in recall or recognition, or (*b*) in subsequent behaviour. It was not clear, however, what caused effect (*a*) or (*b*) to be more marked in some subjects than in others.

One of the main disadvantages of using a recognition test was its inability to predict *what* a subject had actually perceived in the display. In many cases, recognition seemed to be based on memory for visual form rather than for meaningful content. Vernon (1945) has shown how individuals sometimes clearly visualize graphical material without translating the images into the information the graph was intended to convey. It is thus hardly surprising that responses in recognition tests and those in tests of 'behaviour' are so little related.

Similar factors could not explain, however, why some subjects were not influenced by the propaganda although they could recall and understand it. Nor, from a study of the literature, is there a comprehensive theory upon which all our results could be founded. Bergson 1913, and later McDougall (1923), distinguished between two ways in which the past '. . . appears to be stored up', postulating motor mechanisms making use of the past in action, and 'pure' memory being the ability to call up the image of an experience. The term 'use of the past in action', while at first sight appearing to resemble the response of our subjects who used the information, was actually envisaged by Bergson as comparable to habit formation expressed, for example, in verbal repetition habits where the memorization was exclusively dependent upon specific motor mechanisms. It is thus hardly comparable to our groups who could not verbalize but who were able to extract the information from the propaganda for use in another task. A different theoretical assumption has been made by Katona (1940) and Szekely (1950*a*, *b*), who demonstrated how information could either be recalled or used in relation to problem solving or mechanical tasks respectively. They suggest that the difference which exists is not solely one of memory function but is due to different learning processes being involved at the outset. The extent to which disparity between the tests of recall and tests of actual behaviour may be interpreted in terms such as these cannot at present be estimated. It will first be necessary to consider in more detail the conditions which determine a response to propaganda. These will be examined in the second part of this paper.

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A STUDY OF CHILDREN'S INTERESTS AND COMPREHENSION AT A SCIENCE MUSEUM

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As part of an attempt to increase the attractiveness and usefulness of a Children's Gallery in a Science Museum, an inquiry was made into the numbers and types of children who visit it, their behaviour there, and the extent to which they comprehend what they have seen. Methods of investigation included a sampling survey of attendances at different times of day and periods of the year, stop-watch recording of times spent at different exhibits and charts of movements, and an oral questionnaire dealing with the background of one hundred children and their understanding of selected exhibits. The data throw some light on the evolution of genuine scientific interests (mainly in boys) from superficial curiosity, and on suitable and unsuitable exhibits and methods of display and labelling.

I. Introduction

Every year some 300,000 children visit the Science Museum, South Kensington, in addition to some 70,000 who come in organized school parties. In 1952, the advice of the senior author was sought on how to increase the attractiveness and usefulness of the Museum to such children, and an investigation was then undertaken by the junior author in an attempt to elicit some of the psychological factors involved. This study was confined in the first instance to the Children's Gallery—a large basement room containing some thirty groups of varied exhibits designed to be interesting and comprehensible to children. The Gallery aims both to give children a simple introduction to the theme of Science in Daily Life, and to provide a place which will attract them sufficiently to reduce the amount of disturbance they might otherwise cause to more serious students in the main Galleries.

Though many efforts have been made to develop Museum services for children (notably at the Geffrye Museum, the Natural History Museum, at Haslemere and at Glasgow, as well as abroad), little scientifically controlled research has so far been reported. In one study at the Cambridge Museum of Archaeology and Ethnology, A. S. Wittlin (1949) demonstrated the greater effectiveness of a carefully structured exhibit over an old-fashioned hotch-potch display, by conducting an informal oral questionnaire with visitors. Very relevant is the large amount of American work on readability of educational materials, together with studies by B.B.C. staff and by the senior author (Vernon, 1951) of the intelligibility of educational broadcasts. These have demonstrated the extent to which children and adults of average or poor education fail to comprehend printed matter or talks, even when specially designed for them. The main hindrances included difficulties of vocabulary and sentence structure, abstract or uninteresting content, poor organization or structure, and absence in the readers or listeners of technical background necessary to an understanding of scientific, economic or similar subjects. A cursory inspection of the labels in the Children's and other Museum Galleries suggests that many of these might likewise fail in their purpose of explaining the exhibits, except to persons with some scientific education.

Three main techniques of investigation were applied, in addition to informal observation and questioning:

(1) A sampling survey of the numbers, ages, etc., of visitors to the Children's Gallery at different times of day and periods of the year.

(2) Unobtrusive recording with stop-watch of the behaviour of fifty children in the Gallery.

(3) Application of a systematic oral questionnaire to 100 children at the end of their visits.

II. ATTENDANCES

The numbers of persons entering the Children's Gallery were counted during six quarter-hour periods on two days falling under each of the following headings:

(*a*) Summer term weekday.
(*b*) Autumn term weekday.
(*c*) Summer holiday.
(*d*) Christmas holiday
(*e*) Bank Holiday.
(*f*) Fine Saturday.
(*g*) Wet Saturday.
(*h*) Sunday afternoon.

The visitors were classified by sex and—approximately—by age into: adult, 13–16 years, 6–12 years, less than 6 years. Comparisons of the numbers of the two sets of forty-eight occasions gave an index of the reliability of the sampling. The distributions were mostly skewed (i.e. many small attendances, few large ones), and it was considered that tetrachoric correlations, corrected by the Spearman–Brown formula, would provide the most appropriate, even if rough, reliability coefficients. These coefficients are shown along the diagonal in Table 1. In addition, the figures for each type of person in the two sample periods were combined and intercorrelated, to indicate the extent to which high attendances by one type go with high attendances by other types.

Table 1. *Reliabilities and intercorrelations of numbers of adult and child visitors on different occasions*

	Men	Women	13–16 boys	6–12 boys	0–5 boys	13–16 girls	6–12 girls	0–5 girls
Men	**0·95**	0·69	0·55	0·85	0·85	0·68	0·83	0·57
Women	0·69	**0·89**	0·34	0·86	0·81	0·79	0·82	0·84
13–16 boys	0·55	0·34	**0·45**	0·62	0·48	0·54	0·52	0·34
6–12 boys	0·85	0·86	0·62	**0·97**	0·77	0·68	0·87	0·61
0–5 boys	0·85	0·81	0·48	0·77	**0·98**	0·26	0·56	0·60
13–16 girls	0·68	0·79	0·54	0·68	0·26	**0·76**	0·84	0·70
6–12 girls	0·83	0·82	0·52	0·87	0·56	0·84	**0·89**	0·76
0–5 girls	0·57	0·84	0·34	0·61	0·60	0·70	0·76	**0·90**

The mean reliability of 0·85 indicates that the two samples provide a reasonably stable count of the numbers at these times of day and periods of the year. The intercorrelations suggest:

(*a*) The presence of a strong 'general factor'; on any occasion when any one group shows a high attendance, other groups tend to be large also.

(*b*) That older boys are much the most independent from other types, as well as being the most irregular in their attendances.

(*c*) That 6–12-year children chiefly tend to come with adults or in groups together, and children under 6 with an adult or an elder child of the same sex. The statistical data, however, only show that the attendance habits of these groups are similar; and further direct observation would be needed to show how parties of visitors are commonly made up.

It was decided to group the visitors under four main types and to graph their attendances by time of day on each of the 8 days. Table 2 lists the numbers recorded per two quarter-hour periods around the given times. From these figures, totals were estimated for each type of day. It was desired to get a picture of the average attendance at different times on a typical or average day, and to check the overall figures by comparing them with the Museum's total. Most of the 300,000 children coming annually probably enter the Children's Gallery, though a minority may go to the Main Galleries only. With this purpose, the year was somewhat arbitrarily split into 80 days of type (*a*), 110 of type (*b*), and so on; winter and early spring days were judged equivalent to autumn ones. These numbers are referred to in Table 2 as the multiplying factor. Though there are obviously several dubious assumptions in these calculations, the grand total of 229,000 children per year is just about what might be expected.

Table 2. *Attendances in two quarter-hour periods on different types of day*

A. = men and women; B = 6–16-year boys; G = 6–16-year girls; C = children under 6.

Time	People	(*a*) Summer term	(*b*) Autumn term	(*c*) Summer holiday	(*d*) Autumn holiday	(*e*) Bank holiday	(*f*) Fine Saturday	(*g*) Wet Saturday	(*h*) Sunday afternoon	Whole year
10.15 a.m.	A	0	14	21	15	29	24	32	—	
	B	2	4	32	34	8	22	21	—	
	G	0	0	7	1	7	4	5	—	
	C	0	0	8	3	4	2	8	—	
11.30 a.m.	A	27	12	54	15	127	26	89	—	
	B	21	2	41	36	36	13	39	—	
	G	8	3	13	5	33	3	18	—	
	C	0	4	8	3	28	12	14	—	
1.30 p.m.	A	24	18	67	23	116	16	58	—	
	B	19	1	50	22	55	15	31	—	
	G	7	6	18	2	38	3	14	—	
	C	4	2	9	3	32	1	8	—	
3.30 p.m.	A	43	30	110	25	249	53	139	182	
	B	18	12	132	44	70	32	59	81	
	G	8	0	46	13	67	16	26	33	
	C	3	3	10	3	48	16	31	41	
4.30 p.m.	A	42	32	111	12	284	131	119	200	
	B	27	6	78	4	65	59	66	52	
	G	8	0	27	4	53	11	29	48	
	C	3	3	17	8	53	24	28	40	
5.30 p.m.	A	14	6	42	3	183	18	68	110	
	B	5	1	18	6	44	7	21	44	
	G	3	0	13	0	27	3	23	29	
	C	4	0	6	0	30	5	6	22	
Whole-day estimates	A	435	310	1140	315	2640	710	1400	1210	249,000
	B	275	70	1000	420	770	395	660	440	133,500
	G	100	30	350	75	640	110	310	260	54,000
	C	40	40	160	60	540	165	260	260	41,500
All persons		850	450	2650	870	4590	1380	2630	2170	478,000
Multiplying factor		80	110	40	25	3	26	26	53	—
Ratio BGC/A		0·95	0·45	1·32	1·76	0·74	0·94	0·88	0·79	0·92

Fig. 1 gives a smoothed graph of numbers of visitors on a hypothetical typical weekday.

The most striking features of Table 2 and Fig. 1 are the enormous variations on different types of day, and the finding that adult visitors normally exceed children of all ages. On most days attendances rise to a fairly stable period from about 11.30 to 2.0, and then climb to a peak around 4.0. The highest attendances are on Bank Holidays and Sunday afternoons. At peak periods over 200 enter the Gallery per quarter hour, and there is considerable overcrowding.

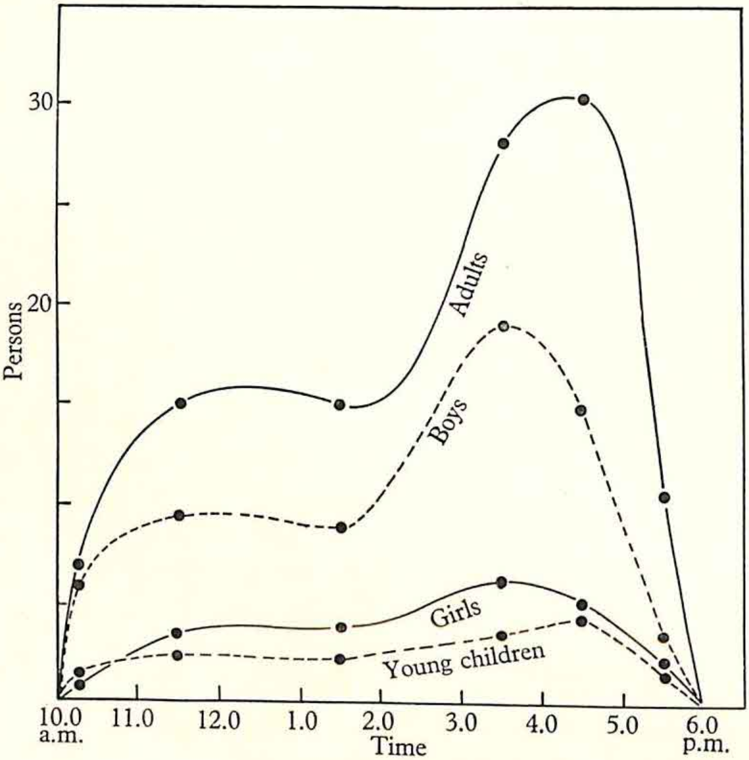

Fig. 1. Visitors entering the gallery per quarter-hour on an average weekday.

The last line of the table shows that children are in a majority only during holiday weekdays. Many of the adults come in order to bring children, and doubtless serve a valuable function in explaining the exhibits to them. But an appreciable proportion come on their own, possibly because they are wandering round the Museum aimlessly, or possibly because their level of appreciation and understanding is often similar to that of the children for whom the Gallery was designed.

III. Records of children's behaviour

A plan of the Gallery was prepared, and on this were recorded the times spent by individual children or pairs at each exhibit. Charts were thus obtained for thirty-five boys and fifteen girls who came unaccompanied by adults. This method was suggested by Marston's (1925) studies of children, where he related types of behaviour in a museum to assessments of their extraversion-introversion. Though our sample is small, it should yield some indication of the attention-catching value of each exhibit. Table 3 lists the exhibits in order of total child × time spent. It also gives the numbers who looked at each exhibit even for a second or two, and the numbers (out of 100) who—in a later inquiry—mentioned each exhibit as their favourite.

It may be seen that less than half the exhibits are looked at by 50% or more of the

Table 3. *Time spent, numbers viewing and numbers choosing each exhibit as favourite*

D = dioramas or models; A = apparatus or specimens; W = working models, etc.

Exhibits	Total min. spent by 50 children	Nos./50 looking	Favourite nos./100
Communications over long distances (D, A, W)	132·13	43	12
Methods of lifting, pulleys, jacks (W)	84·53	39	1
Experiments in a vacuum (A, W)	65·69	26	8
Stage-lighting (D, W)	54·38	31	2
Transport through the ages (D)	49·16	29	19
Photoelectric door (W)	48·90	39	12
Forces in a bridge when engine crosses (D, W)	39·19	36	—
Photoelectric burglar alarm (W)	35·46	37	9
Lighting through the ages (D)	33·84	32	9
Eclipse of the sun (W)	24·83	25	2
Sources of power, Archimedes screw, etc. (D, W)	23·73	26	4
Measurement of time (A, W)	21·99	19	2
Photoelectric sorting of objects by colour (W)	17·26	22	—
World clock (W)	14·08	28	—
Photoelectric parcel counting (W)	11·88	21	3
Magnification by lenses (A)	10·56	18	—
Making tools, hand nails, flints (D, A)	10·43	19	3
Street lighting (D)	7·76	16	5
Photoelectric control of street lamps (W)	7·65	17	—
Expansion of heated metals (W)	7·31	4	1
Mining (D)	7·07	21	—
Methods of transport, ships, carriages (D)	4·02	15	—
Old and modern kitchens (D)	4·00	15	1
Roman pottery kiln (D)	1·71	16	—
Roman villa (D)	0·15	3	—
Illuminated photographs, various	0·05	4	—
Manufacture of pottery (A)	0·00	0	—
Pressing buttons, turning handles, seeing how things work			7
Total			100

children, and that less than half are studied for more than 1 min. by the average child who looks at them at all. This is much what one would expect of any museum visiting. However, these average figures conceal a wide range of variation. A proportion of children do spend 5–10 min. or more at particular exhibits that arouse their interest. Almost always these are of the working model rather than the static diorama type. But a larger proportion tend to flit from one thing to another, stopping to press buttons or turn handles, and treating the Gallery more as an amusement arcade than as a source of scientific information.

The average time spent in the Gallery by boys was 22·4 min. (ranging up to 45 min.), and by girls 14·6 min. This weaker interest among girls is borne out by our finding that only 40% as many girls as boys come at all.‖

IV. ORAL QUESTIONNAIRES

The investigator buttonholed sixty-eight boys and thirty-two girls (unaccompanied by adults) as they were leaving, and asked them to answer some questions in a quiet adjoining part of the main Galleries. No precise scheme of sampling could be devised, but as far as can be judged these children, picked at random, are not unrepresentative of all 7–16-year visitors. Table 4 gives the age and school distribution.

Clearly the greatest proportion consist of secondary modern and older primary boys, and grammar-school girls, whose scientific interests and education possibly resemble those of younger boys. Since it is hardly possible to envisage exhibits which would be suitable for children of all ages and all educational levels, it is probable that 11–14-year boys, who do not get specialist science teaching at school, should be regarded as the 'target', or at least as the modal, audience. Grammar-school boys often indicated that the exhibits were too childish for their tastes.

Table 4. *Ages of sample and schools attended*

Age (years)	7–8	9	10	11	12	13	14	15	16
Nos.	3	6	9	10	24	20	15	11	2

Schools	Boys	Girls
Junior	12	6
Preparatory (7–11)	5	1
Preparatory and Public (11–12 +)	7	2
Secondary Modern	23	7
Secondary Technical	8	0
Grammar or High School	13	16
Total	68	32

Nineteen were visitors to London from various parts of the United Kingdom or abroad, and twenty-four came from Kensington or adjoining boroughs—nine of these on foot or by bicycle. The remainder—more than half—came from more distant parts of London, as far as Croydon, Hendon, Chingford and Woolwich, by means of buses, tube [illegible] both. Their average return fare would amount to about 1*s*. Although we have commented on the superficiality of the scientific interests of many children, the trouble and expense they incur in getting to the Museum are noteworthy. They are not merely local residents visiting a fun fair. There is little else in the neighbourhood, except other museums, to attract children.

In answer to questions about their first visit:

28 were brought by parents or relatives
14 by friends
5 by brothers or sisters
15 in school, Scout or Guide, parties
35 were told about it by parents, friends or school
3 found it for themselves.

(Only nine of the two latter groups were girls.) The median age claimed for first visit was 10–11 years, and the great majority came at between 7 and 13 years. The numbers of visits claimed (obviously a rather unreliable datum) were:

Visits	Boys	Girls
1	14	15
2	15	8
3–4	10	6
5–9	15	2
10–19	8	1
20–40	6	0

It would appear again that the majority of boys are habitués who, once they have been brought or had their interest aroused by an adult or friends, return, sometimes with considerable frequency. Girls indulge in repeated visits much less frequently than boys. When asked why such visits were made, 219 reasons were given, which could be classified under the following headings:

Helps school work	66
Mechanical and scientific interest	82
Amusement	69
Somewhere to spend the day	2

In spite of the unreliability of children's introspections, this suggests that a majority do possess at least an incipient serious purpose in coming.

After asking children to name their favourite exhibit (cf. Table 3), they were given a choice of three out of five and told to explain 'how they work'. Answers were graded:

+2	fairly good conception
+1	some conception
0	don't know
−1	confused
−2	very confused

Inadequacy of expression and ignorance of technical terms were not considered. Though there is, of course, some subjectivity in marking, fair consistency was reached (as in the writer's study of educational broadcasts) by listing beforehand the essential points to be met by a +2 or +1 answer. Eleven children (9–13 years) could not say anything about any of the five exhibits, and several others attempted only two exhibits; hence the total answers are 212 instead of 300.

Table 5. *Results of oral comprehension test*

Exhibit	Marks					Total answers	Total +answers	Mean score for those attempting
	+2	+1	0	−1	−2			
Photoelectric door	3	38	16	4	7	68	41	+0·38
Forces (engine)	7	17	6	4	18	52	24	−0·17
Pulleys	11	5	15	3	10	44	16	+0·09
Burglar alarm	11	8	8	2	1	30	19	+0·87
Archimedes screw	3	6	6	2	1	18	9	+0·44
Total	35	74	51	15	37	212	109	

The burglar alarm seems to be the most comprehensible, to those who attempt it. The photoelectric door and Archimedes screw also get a majority of plus answers, though very few children have a good grasp of how they work. The pulleys, though one of the most patronized exhibits, probably illustrate too difficult a principle. They and the engine have particularly complex labels. Some resemblance may be noted between these results and the votes for favourite exhibits (Table 3), suggesting that children do not particularly like exhibits which they do not understand, merely because they can play about with them actively.

Though these results are limited to a few exhibits, they show that the oral comprehension test is a feasible technique for evaluating both the exhibits and the amount that children gain. Unsolicited remarks were useful also. Thus there was frequent mention of the length and complexity of some of the labels, and many children suggested that they would like somebody to be there to explain things to them.

V. DISCUSSION

The children who visit the Museum on their own, or in family groups (as distinct from school parties), vary enormously in age, intelligence and scientific education and interests. However, it would appear that many 11–14-year-old boys, even if they have little or no science in school, are sufficiently curious about the applications of physical science to justify a Gallery like the present one, in which a series of simple exhibits are provided to illustrate basic principles and techniques and the broad lines of technological suitable for children of all ages and all educational levels, it is probable that 11–14-year boys, who do not get specialist science teaching at school, should be regarded as the 'target', or at least as the modal, audience. Grammar-school boys often indicated that the exhibits were too childish for their tastes.

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Forces (engine)	7	17	6	4	18	52	24	−0·17
Pulleys	11	5	15	3	10	44	16	+0·09
Burglar alarm	11	8	8	2	1	30	19	+0·87
Archimedes screw	3	6	6	2	1	18	9	+0·44
Total	35	74	51	15	37	212	109	

The burglar alarm seems to be the most comprehensible, to those who attempt it. The photoelectric door and Archimedes screw also get a majority of plus answers, though very few children have a good grasp of how they work. The pulleys, though one of the most patronized exhibits, probably illustrate too difficult a principle. They and the engine have particularly complex labels. Some resemblance may be noted between these results and the votes for favourite exhibits (Table 3), suggesting that children do not particularly like exhibits which they do not understand, merely because they can play about with them actively.

Though these results are limited to a few exhibits, they show that the oral comprehension test is a feasible technique for evaluating both the exhibits and the amount that children gain. Unsolicited remarks were useful also. Thus there was frequent mention of the length and complexity of some of the labels, and many children suggested that they would like somebody to be there to explain things to them.

V. DISCUSSION

The children who visit the Museum on their own, or in family groups (as distinct from school parties), vary enormously in age, intelligence and scientific education and interests. However, it would appear that many 11–14-year-old boys, even if they have little or no science in school, are sufficiently curious about the applications of physical science to justify a Gallery like the present one, in which a series of simple exhibits are provided to illustrate basic principles and techniques and the broad lines of technological development. The main Galleries can, and do, contain valuable material which can be related to school science courses. But the Children's Gallery should not be tied to any syllabus; rather it should appeal to their sense of wonder at natural phenomena and mechanical achievements, to their desire to know how things work, and to manipulate things themselves. The children who come already thoroughly enjoy themselves, and many more would do likewise if there were more publicity, more commodious conditions (such as are envisaged in the near future), and if more of the exhibits were more readily intelligible. At the moment the activities of a large number, particularly of younger children, are very superficial and unrelated to science. Yet they tend to return many times, and it is reasonable to assume that the button-pressing and gaping do often represent the early stages of evolution of a genuine interest. Moreover, they do usually want to understand, even if they lack the perseverance or the reading capacity or educational background to make proper use of the opportunities offered. We have said little about girls, but our evidence clearly suggests that fewer of them than of boys evince the same kind of scientific curiosity, and then not until a distinctly later age.

In spite of the small samples and other inadequacies of the data, some light has been thrown by the present inquiry on the most profitable kinds of exhibit. In assessing these it is desirable to take into account not only time spent, ratings for popularity and level of comprehensions, but also the distribution of responses. For example, expansion of heated metals is low on our list (Table 3), but is surely a worth-while exhibit in that the few children who looked at it usually spent as much as 2 min. doing so. Working models clearly have the greatest pull, though some dioramas closely related to daily life interests (e.g. transport and lighting) are often high in popularity and time spent. Others, however, are apt to appear antiquated or grotesque. Probably the bottom quarter of exhibits on our list could be eliminated without appreciable loss, though it would be desirable to confirm this by more extensive observations. Obviously the range of exhibits which children can be allowed to handle or manipulate is limited; but the glass cases enclosing most of them do tend to detract from the sense of reality, and this 'behind-glass' feeling might be reduced by more use of curved glass panels.

Finally, we would stress that the planning of a museum to be of value to children is intimately bound up with child psychology. It should be based on knowledge that the psychologist could contribute of the origins and development of their interests and of the range of their capacities, rather than on subjective adult judgements. Yet at the moment the psychologist can give little detailed advice on these points, and there is very great scope for further research—research which would be welcomed by most museum authorities.

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'ABNORMAL BEHAVIOUR' INDUCED BY NITROUS OXIDE

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Psychological changes induced in normal volunteers by inhalations of nitrous oxide are described. Some of these changes resembled psychiatric symptoms, for example illusions and hallucinations, perseverations, and 'dissociated' states.

There was no relation between individual differences in the number of changes reported, ratings for emotional stability, and cognitive impairment attributable to the drug.

Possible relations between normal, drug-induced, and other forms of abnormal behaviour are discussed.

I. INTRODUCTION

It has long been known that certain drugs which act on the central nervous system can temporarily induce a variety of unusual behaviour phenomena in normal subjects. The resemblance between such phenomena and those characteristic of psychiatric disturbances was already noticed by Kraepelin (1892), who thought that the study of drugs might therefore throw light on the nature and etiology of mental illness.

Among more recent exponents of a similar approach have been Lindemann & Malamud (1934) working with sodium amytal, cocaine, hashish and mescaline, Lindemann (1935) with adrenaline, Newman (1935) with alcohol, Levin (1946) with bromides, Bromberg (1934) with marihuana, Stockings (1940), Klüver (1942), Hoch (1951) and McKellar (1954) with mescaline, DeShon, Rinkel & Solomon (1952), Hoch, Cattell & Pennes (1952*a*), Elkes, Elkes & Bradley (1954), and Sloane & Lovett Doust (1954) with lysergic acid, and Hoffer, Osmond & Smythies (1954) with adrenochrome. Various aspects of the possible relation between such 'synthetic' behaviour abnormalities and psychopathology have been discussed by Mayer-Gross (1951), Osmond & Smythies (1952), and Hoch, Cattell & Pennes (1952*b*). Until recently, however, there has been relatively little experimental follow-up in the laboratory. Most of the published studies have been qualitative and clinical, and the subjects often confined to psychiatric patients and few in number. Few investigators have given systematic descriptions and classifications of the behaviour phenomena induced, or have made statistical analyses of the frequencies with which they occur and of their relations with other aspects of behaviour.

The data to be presented here were gathered in the course of experiments (Steinberg, 1954) with the anaesthetic drug nitrous oxide (N_2O). The precise mechanisms through which anaesthetics act are imperfectly understood; available evidence has recently been reviewed by Butler (1950). Though familiar as an adjunct to psychotherapy, nitrous oxide does not appear to have been extensively considered hitherto for the purpose of experimentally inducing behaviour abnormalities as discussed above. Numerous clinical and incidental observations of its effects on behaviour have, however, been reported, for example by James (1905), Zador (1928), McKinney (1932) and Marshall (1937, 1938).

The aim of the present paper is to describe effects of nitrous oxide on a reasonably large sample of normal subjects, to classify these effects, examine their respective incidence and finally, to explore the relation between them and other aspects of the subjects' behaviour. A fuller description of the investigation is contained in a thesis by the author (Steinberg, 1953).

II. METHOD AND PROCEDURE

Data were obtained in the course of two series of experiments:

(1) *Main experiments*

Fifty student subjects, 36 male and 14 female, mean age 21·5 years, S.D. 3·4 years, carried out a series of timed cognitive performance tests, lasting about half an hour, first for practice and then while inhaling through a face mask a scented mixture of 30% nitrous oxide and oxygen. A further 50 subjects, matched for age and sex, served as controls; their treatment differed from that of the experimental subjects only in that they inhaled scented air instead of nitrous oxide. The experimenter: (i) recorded any incidental behaviour, including verbal behaviour, in shorthand; (ii) obtained a written introspective account as soon as the inhalation was over; and then (iii) presented orally to each subject a prepared check-list mentioning some sixteen psychological and physiological symptoms commonly encountered with this and similar drugs. Finally each subject was asked to state what he thought he had been inhaling.

(2) *Miscellaneous additional experiments*

Seven subjects attempted, but did not complete, the experimental procedure in the main experiments, owing to loss of consciousness or other unfavourable reaction to the drug. A further 12 subjects were given varying experimental treatments, including higher concentrations of nitrous oxide in oxygen. For each of these subjects the experimenter again (i) recorded incidental behaviour, and (ii) obtained written introspections; the check-list was not, however, used.

III. RESULTS

A great variety of abnormal phenomena occurred during inhalation of the drug; except for some of the physical complaints, they usually subsided almost immediately after administration ceased.

All figures quoted refer to the main experiments and not to the miscellaneous additional experiments; illustrative material is taken from both.

(1) *Incidence of symptoms*

Table 1 shows the proportion of subjects who reported each particular kind of psychological and physiological change. The much higher incidences reported in reply to check-list questions, as compared with the spontaneous introspections, were probably partly due to the suggestion effect of the questions and partly to most subjects' lack of familiarity with the process of introspecting; possibly also the check-list questions may, by their specificity, have helped to counteract any retrograde amnesia induced by the drug. Moreover, though the control group showed analogous discrepancies between spontaneous and check-list reports, the proportion of subjects who admitted to behaviour changes in response to check-list questions was altogether very much smaller than in the experimental group, and many control subjects also qualified their replies by pointing out that the symptoms had been very slight or only due to 'nerves'. Table 2 gives the frequencies with which some additional phenomena, not explicitly covered in the check-list, were mentioned in the introspections. It may be noted that the control group referred mainly to minor discomforts attributable to the apparatus, which seem to have been much less noticed in the experimental group.

The tables show that the frequency with which different symptoms were reported in the experimental group varied considerably. None was universally experienced by all subjects, but unsteadiness of movement, tingling, numbness, dizziness, auditory disturbances, and, above all, phenomena suggestive of various aspects of personality dissociation, such as difficulty in concentrating, 'dreaminess', feelings of being 'distant' and of losing control over one's actions, appear to have been extremely common.

Table 1. *Frequency with which different kinds of symptoms were reported by the experimental and control groups*

	Percentage of subjects reporting					
	Experimental group			Control group		
Symptom	In answer to check-list $N=49$*	Introspections $N=50$	Check-list and introspections combined $N=50$	In answer to check-list $N=36$*	Introspections $N=50$	Check-list and introspections combined $N=50$
'Compulsive' laughter	12	10	20	0	0	0
Unsteadiness of finer movements	80	16	80	33	0	24
Impaired control over grosser movements	43	12	46	6	0	4
Tingling	67	30	68	6	0	4
Numbness	51	22	50	3	0	2
Visual disturbances	35	4	38	6	2	6
Auditory disturbances	49	24	52	14	6	14
'Dreaminess'	88	18	86	31	2	24
Difficulty in concentrating	92	46	92	39	18	34
Difficulty in talking	35	4	34	6	0	4
Difficulty in writing	51	10	50	8	0	6
Impaired control over whole situation	69	2	68	22	0	16
Memory disturbances	59	16	62	31	2	24
General effect of inhalation:						
Pleasant	45	12	44	28	4	22
Unpleasant	33	0	32	11	0	8
Neutral	20	2	20	56	0	40
Pleasant and unpleasant	2	0	2	0	0	0
Euphoria	69	20	72	14	6	14
Dizziness	53	10	52	14	8	12
Nature of gas:						
Drug	86	6	84	14	0	10
Air	2	0	2	69†	2	52
Both	6	4	6	0	2	2
Doubtful	6	0	6	17	6	18

* Because of shortage of time, the check-list was not given to all subjects.

† Includes five doubtful at first.

A 'symptom score' was computed for each subject by assigning one mark for each kind of symptom he had reported in the introspections, and/or in reply to the check-list. As is shown in Fig. 1, individual differences in the experimental group were considerable. The smallest number of symptoms reported by any one subject was 8 and the largest 22. In the control group the scores were much smaller, and there is very little overlap between the two distributions. The differences between the two groups were equally striking when the number of symptoms per subject was computed from the information given in the introspections alone. Three of the control subjects were under the impression that they had been given a drug, and the numbers of symptoms they reported were among the largest in the control group.

(2) *Examples of behaviour abnormalities*

The examples given below have been selected partly for their intrinsic interest, and partly for their particularly suggestive resemblance to psychiatric symptoms. Four broad categories of phenomena will be distinguished, a classification which, needless to say, is by no means absolute or exhaustive. Disturbances of the perception of time, of which a large

Table 2. *Additional symptoms described in introspections*

Symptom	Percentage of subjects reporting: Experimental group	Percentage of subjects reporting: Control group
Discomfort from mask	18	28
Difficulty in breathing	2	26
Smell (unpleasant)	2	8
Smell (pleasant)	—	4
Sleepy, tired	12	8
Weak, faint feeling	14	—
Sick feeling	4	—
Hot, flushed	10	—
Far away, detached	36	—
Careless	38	—
More confident	6	—
Cheated in tests	8	—
'Automatic' behaviour	12	—
Some tests seemed easier than before	22	—
Performance improved in some tests	56	—
Vivid imagery	2	—
Wanted to talk, cheerful	22	—
Tendency to repeat oneself	12	—

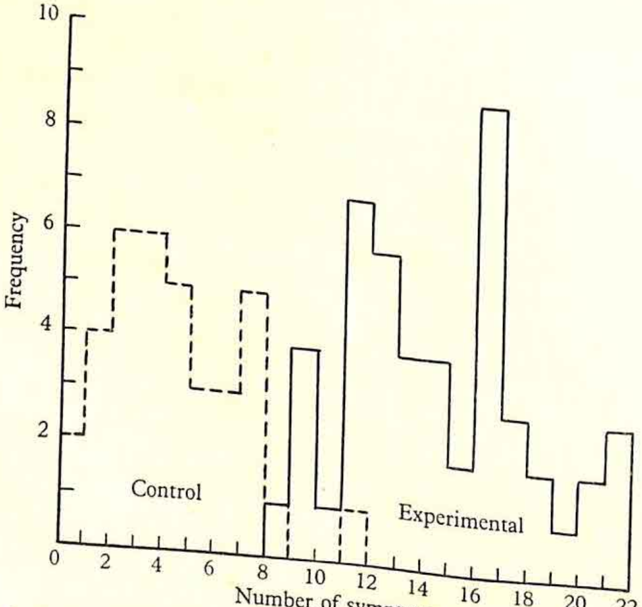

Fig. 1. Frequency distributions of the number of symptoms per subject reported in the experimental and control groups.

number were reported, have been omitted altogether, as they have been discussed in detail elsewhere (Steinberg, 1955). The concentration of nitrous oxide, if other than 30%, is given where appropriate in brackets.

(a) *Illusions and hallucinations*

The majority of these were auditory, possibly partly because it was found difficult to eliminate all extraneous sounds from the laboratory. The anaesthetic machine itself was almost inaudible, but a kettle of water had to be kept boiling throughout.

Music was reported by a number of subjects, though none was audible to the experimenter at any time during experiments. For example, one subject (50% N_2O) reported during a long experiment lasting 1 hr. 25 min. that a Mozart or Beethoven opera was being played 'to and fro' in an adjoining building. She referred to this altogether nine times during inhalation and on one occasion described it as 'an hallucination'. Another subject said that he 'heard a noise of what was probably a kettle boiling, but which sounded like an orchestra playing a monotonous tune'. The 'wireless' was referred to by several subjects. One reported that he heard 'a wireless with a programme on—a comedian cracking jokes and someone singing', and another remembered hearing rhythmic drumming which seemed to him to get louder and louder, 'in a manner similar to that used with effect on the films'.

Sometimes the illusions assumed what might be called a paranoid character. One of the subjects (50% N_2O), whose report is reproduced in full later, thought that the workmen on the roof of a building opposite were pointing and laughing at her. Another subject felt as if someone else far away was himself and was laughing at him. Various remarks were attributed to the experimenter which she had not in fact, made, and the content of which sometimes seemed readily explicable in terms of emotions of the moment. For example, a subject (50% N_2O) who during inhalation had been complaining of being much disturbed by the drug, suddenly removed her mask half way through the experiment saying that she took the experimenter's remark 'You can come off now', which the experimenter had not in fact made, to mean 'take off the mask now', which the experimenter had not said either. Several subjects thought the experimenter was laughing. One of them 'heard' her humming and another sighing.

One subject (50% N_2O) reported a curious visual phenomenon: on his right, where a friend of his was actually standing with his back to the wall, only a pair of legs remained; the upper half of the body had disappeared and the wall was visible behind. Similarly, the surface of the table at which the subject was sitting disappeared and only the paper and pencil remained, 'suspended in mid air'. Another subject reported that for a very short time he was unable to see, as prior to an attack of migraine to which he was prone.

One illusion involved the sense of pain. The subject reported that inhalation reminded her of the last time she took gas at the dentist's. She thought that she was hearing an alarm bell, as of an ambulance, and then felt as if she were going to have a tooth extracted. At the same time, however, she 'knew' that she was not actually at the dentist's. 'The sound of the alarm bell increased, and then died away again—at which point I felt the pain of extraction. I removed the mask because previously the pain had increased to almost unbearable intensity'. The subject actually did remove the mask 2 min. 25 sec. after beginning inhalation.

(b) *Irrelevant or exaggerated thoughts and emotions*

An outstanding characteristic of these phenomena was the force and persistence with which they seemed to be experienced, even though their content might be relatively trivial. Especially when higher concentrations of nitrous oxide were given the subjects' conversation often flowed very easily and irrelevantly by free association, usually taking the original cues from features of the experimental situation or their ordinary day-to-day life and occupation. For example, a zoologist talked at length about birds and insects, and students of psychology tended to express themselves in the technical language of their subject, especially in psychoanalytic terms. Sometimes the content showed more than ordinary lack of restraint.

Occasionally behaviour indicated aggressive tendencies, as when a subject reported that he felt hostile toward the experimenter, and another (40% N_2O) noisily objected to some minor extraneous noises and shook his fist so violently that he knocked a hole into an adjacent fibre-board wall. Some subjects became euphoric, and irresponsible, variously describing themselves as feeling pleased and happy, 'gay', 'less shy than usual', terribly sorry that time was going as quickly and that the experiment was over so soon'. However, overt laughter, as can be seen from Table 1, was rare, possibly because subjects were mostly kept busy with tests and were given no social stimulation. Several subjects, however, reported that they had had to suppress tendencies to laugh. One subject, who actually did laugh loudly, commented afterwards that although he knew he was laughing, he 'didn't feel facetious'—a disjunction between affective state and overt behaviour somewhat reminiscent of the well-known phenomenon of 'sham rage'. A few subjects who were heard laughing by the experimenter during the inhalation, subsequently appeared quite unaware that they had done so.

Nineteen subjects in the main experimental group of 50 commented in their introspections that they became less concerned over accuracy in the cognitive tests. Several reported that some or all of the cognitive tests seemed easier than before, although actual performance was usually found to have deteriorated. One subject thought he was 'doing particularly fine' in an arithmetic test, though in fact he had only attempted three sums and got two of them wrong. One or two tried to obtain higher test scores by cheating, for example by telling the experimenter that they had inserted a greater number of balls into a manual-dexterity test apparatus than was the case. One subject wrote in his introspections: 'In the track tracing...I found myself "cheating" quite unconsciously and carelessly.' Although, as is shown in Table 1, pleasant experiences were reported more frequently than unpleasant, a very few subjects were visibly distressed and anxious during inhalation and one said he felt 'panicky'. The male subject (35% N_2O) whose full introspections are reproduced later, experienced 'flashes of insight' and made the 'special discovery' that 'everything is as it is'.

(c) *Perseverations*

Occasionally, subjects interpreted environmental stimuli in a perseverative manner. For example, one reported that during inhalation (35% N_2O) he had heard the experimenter ask several times 'Are you all right; are you all right', when in fact she had only asked it once. More often, however, the subject's own actions were themselves repetitive. Thus the same subject also reported that during the experiment he kept repeating to himself 'I am still here, I am still here...'. A female subject, while inhaling a rather high concentration (60% N_2O) said 'That's all right, that's all right' five times one after the other and with great emphasis. An example of perseverative motor behaviour has already been described elsewhere (Wilson, Crockett, Exton-Smith & Steinberg, 1950). A subject carrying out the manual-dexterity test under 40% nitrous oxide continued with regular and precise movements of his forceps to insert imaginary balls into the apparatus for several minutes after he had been told to stop and had exhausted his supply of balls, and only left off when the gas was withdrawn. As in the case of laughter, some subjects whose behaviour had not actually shown repetition reported afterwards that they had had to suppress tendencies to keep repeating particular words or actions.

Perhaps the most perfect example of perseveration was provided by the female subject whose introspective report is reproduced later. She was under the impression that she had become a gramophone record going round and round and round, endlessly repeating itself.

(d) '*Dissociated*' *states*

Eighteen subjects in the main experimental group and many of those in the other experiments reported feeling 'distant' in some sense. With some this involved a definite notion of another place, e.g. 'back in the Army', but more often it seemed to be a feeling of being far away, but 'nowhere in particular'. One male subject (50% N_2O) reported that 'there was a curious irrelevant appearance in my consciousness', and another (20% N_2O) said he 'felt abstracted'. In some cases these feelings seemed to involve depersonalization. The subject would appear to be watching himself, as in a dream, or parts of his body would seem misplaced or not belonging to him. For example, one subject (35% N_2O) felt far distant, and someone else, '24 years younger', seemed to be performing his tests.

(3) *Specimen verbatim introspections*

Many of the various phenomena described under the four headings above are illustrated by the two introspective reports which are reproduced below exactly as they were written by the subjects. Both subjects were studying psychology, and therefore probably more adept than average at describing their experiences. For purposes of comparison, the introspections of one control subject who was under the impression that he had been given nitrous oxide are also given.

(a) *Female graduate student of psychology*

The subject was given two 5 min. inhalations of 50% nitrous oxide, separated by a pause of 1 min. Her introspections were written during inhalation. As they were somewhat illegible, she transcribed them after the experiment, and added some explanatory comments which are given in brackets below.

First inhalation

Weak feeling in fingers and toes, shoulders—Aware of fast heart beat. Feel very weak, v. slow and difficult to write—Great heart beat. Think I shall collapse. Drumming and buzzing. Writing almost automatic. Tingling of lips. Drumming and buzzing. As if from a great distance (i.e. my writing looked very far away). Drumming and rhythm. (Aware of significance of rhythm as sexual symbol.) Can't help laughing. Everything much clearer but more distant than usual. Contraction across my chest. Can only think of two words ahead instead of 6–12 (Remember difficulty of estimating how many words ahead I could think) As from a great distance. Laughter and contracting from elbow to chest. From a distance and aslant. Can't write keeping my letters together any more.* (Self criticism of 'u' for 'a'.) It does look funny. Can't see Getting quicker quicker quicker Can't help laughing laughing laughing laughing feel like a gramophone record record record ('Passivity feeling'.).

Second inhalation

I still feel rather weak and numb about the extremities, rather distant, my heart is rather fast, there is a big rhythm in the machine and I can't quite control my hands. The rubber† (which touched my knees at this point) is like a big snake and there is big drumming. He is going out (The man in white‡ went out at this point and I felt sexually attracted to him!) And it is difficult to cross my i's and not to write backwards (it looked very left-sloping in the 'distant' view) which must be left-handed or neurotic and I write smaller and smaller almost automatic. (looked like writing through wrong end of binoculars or the pit in the 'Snake Pit'.) I must be getting pas. ('going to pass out') I can't help writing left handed (i.e. sloping backwards). This is my Ph.D.§ This is my Ph.D. (i.e. neurotic symptoms under gas.) it is fun repeating myself. Smaller and smaller smaller and smaller. So funny humming humming like a dance tune from Oklahoma (the one the aunt sang about 'When a fellow asks a girl—I can't say no', but I did not hear the words) Isn't it Rhythm Drumming it is so clear from a distance. Don't know if I can read it close-up. Biting myself (I think I was biting my tongue quite hard.) I can hear close up music now ('close up' was some sort of association with films) Cookhoo Cuckoo I can't spell Dance music and giggling!!

Feel so giggly though can't write properly but I can TALK! Oh how I can talk. Does this go back to early childhood? It's wearing off and my mask is askew.

(*b*) *Male student of psychology*

The subject inhaled 35% nitrous oxide for about 30 min., while carrying out the same cognitive tests as those used in the main experiments. He began writing his introspections a few minutes after the inhalation was over.

I felt as if I were far distant and that the individual performing the tests were someone else, several years younger, say twenty-four years younger. I experienced some early memories, or perhaps the affective tone accompanying past experiences. Throughout, there was a tendency to be sick, and another to go to sleep. The super ego function was intensified in such a way that an actual splitting of the mind seemed to occur, the alternate phases being devoted to 'self' and 'higher self' respectively. This latter functioned critically.

There were flashes of insight, or apparently so, and a desire to stop doing the particular test in order to follow up a special discovery. These discoveries took the form of enlightenment concerning the nature of mind. One such piece of insight was the uncovering of the truth that 'everything is as it is'. Having discovered this, I would try to pursue the implications of the proposition, only to remember that the task in hand was to complete the performance test.

Temporal relations were replaced by spatial ones. When thinking of a childhood experience I myself would at that moment be looking down from far above; if the experience were a recent, everyday one, I would, rather, be taking part in it.

A feeling of numbness persisted. Tactile sensations lost intensity and significance. The tendency to salivate was strong.

I am not convinced of the worthlessness of some of the thoughts that passed through my mind. For instance, the desire to be sick had the concomitant thought, 'This gas makes me sick.' Then I associated this with a visit to the dentist. Dentist plus gas equals sickness. But if the dentist alone had once been the cause of sickness, the experiencing of the gas which called up the image of the dentist would call up the corresponding affect of sickness. The gas might therefore be uncorrelated with the *cause* of sickness. No appreciation of this idea resulted in a diminution of my fear of being sick. As an aside, the 'image' of the dentist was not a visual one; if anything, it was olfactory; it may not have been an image at all, but a 'functional gnosia' from which the psychical concomitants were divorced.

But even what flashes of apparent understanding into 'new truths' there were, these were undoubtedly heavily weighted with a degree of significance which they didn't deserve. The idea occurred that 'a person's behaviour is not necessarily the effect of a certain series of events, but may be the effect of another series of events'. Stated in this way it seems rather obvious, but observers of people's behaviour might find it a salutary guide when deducing causes of behaviour: unwarranted assumptions often result in false conclusions which may be based on otherwise accurate data.

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* The subject's handwriting had deteriorated, and she was leaving letters like 'a' and 'o' partly open.

† Corrugated rubber tubing which connected the subject's face mask with the anaesthetic machine.

‡ A male technician.

§ The subject was at that time searching for a suitable research project for her Ph.D.

(c) *Male medical student*

This subject inhaled scented air while carrying out the tests used in the main experiments. The introspections were written a few minutes after the inhalation was over.

> There was a slight Physical Sensation of dizziness at isolated periods during the test, particularly at the start. There was slight impairment of vision in the right eye, for a time, and this can be deemed to have had no effect upon the result of the tests.
>
> Mental alertness was, if anything, slowed, and I got the impression that I was reacting to the stimuli after an appreciable lag—especially the word 'go' which often seemed to come as a slight surprise. Disorientation was not a significant influence in the tests since it only accounted for 1 sec. or so, within other errors.
>
> The tests seemed more variable this time, some being harder than others. It was as if any natural abilities were emphasized or failings brought to light, e.g. mathematical ability, manual dexterity.
>
> On the whole, they seemed to be of the same standard as on the 6 Dec.

(4) *Relation between individual differences in symptom scores and emotional stability*

The emotional stability of 31 experimental and 37 control subjects was expressed in terms of ratings on a five-point scale made by one or other or two teachers who were experienced raters and well acquainted with these subjects. The experimenter's own ratings, made solely on the basis of the subjects' behaviour in the two experimental sessions, correlated significantly with the teachers' ratings, r being 0·443 for the experimental and 0·641 for the control group. The P values on a 1-tail test, used since a positive correlation was predicted, were less than 0·01 and 0·001 respectively. Only the teachers' ratings were used in subsequent calculations, however, as they were regarded as more trustworthy.

Product-moment correlations were calculated between the symptom scores described on p. 185, and the ratings of emotional stability for both groups of subjects, and were found to be not significantly different from zero.

(5) *Relation between individual differences in symptom scores and cognitive impairment*

Quantitative results were available of all experimental subjects' performance in the cognitive tests both before and during inhalation. From these standard difference scores were computed which were measures of the effect—usually deleterious—which the drug had on each subject's performance relative to the rest of the experimental group. The procedure has been described in detail elsewhere (Steinberg, 1953). A product-moment correlation was calculated between these standard difference scores and the symptom scores, and was again not significantly different from zero. Similar calculations were carried out and a similar result obtained from the control subjects. Further, no relation could be detected, in either group of subjects, between the standard difference scores and the ratings of emotional stability.

IV. SUMMARY AND CONCLUSIONS

The findings described may be summarized as follows:

(*a*) Subjects given nitrous oxide reported various psychological and physiological changes. The incidence of such symptoms was significantly greater than among control subjects.

(*b*) Subjects reported many more symptoms in response to the experimenter's specific questions than they had done in the course of free introspective reports.

(*c*) No single symptom was reported universally by all subjects; various degrees of 'dissociation', unsteadiness of movement, tingling, numbness, dizziness, and auditory disturbances were, however, particularly common.

(*d*) Four categories of symptoms strikingly resembled behaviour abnormalities characteristic of psychiatric disorders: illusions and hallucinations, irrelevant or exaggerated thoughts and emotions, perseverations, and 'dissociated' states. Phenomena of this kind were observed particularly in experiments where relatively high concentrations of nitrous oxide were used.

(*e*) No correlation was found between individual differences in the number of symptoms per subject and ratings for emotional stability or impairment of performance in cognitive tests.

While illustrating the wide differences of behaviour abnormalities possible both within and between subjects, the results described clearly do not justify more than the most tentative interpretations.

For example, as regards individual differences in *degree* of disturbance of behaviour one might on various grounds have expected emotionally less stable subjects to be more disturbed by the drug, both in terms of the number of abnormal behaviour phenomena shown and in terms of deterioration in cognitive efficiency. Thus, in connexion with experimentally induced anoxia, several investigators (McFarland & Barach, 1937; Hertzman, Orlansky & Seitz, 1944) have reported that the behaviour of neurotic patients is particularly liable to be adversely affected. Further, it has repeatedly been shown that performance in tests of cogitive efficiency can discriminate significantly between normal and neurotic subjects (Eysenck, 1947, 1952). The lack of correlation in the present research may be attributable to the crudity of the measures used or to the absence of extremes of emotional instability among the student volunteers. On the other hand, susceptibility to a drug may not be a unitary variable, and different aspects of behaviour may be differentially susceptible in different individuals. This would be consistent with Landis & Zubin's (1951) failure to detect reliable relations between subjective feeling states and cognitive test performance under the influence of two different drugs, and with the absence of correlation between the deterioration of three different kinds of cognitive performance during anoxia reported by Birren, Fisher, Vollmer & King (1946). In the present research the intercorrelations between the measures of relative deterioration in the different kinds of cognitive performance tested were also very small.

As for individual differences in the quality and content of behaviour abnormalities induced by drugs, the attractive though relatively unproved hypothesis has often been put forward that they are determined by the individual's existing and latent behaviour tendencies. For instance, Hoch (1951) has expressed this as: 'The best one can say by way of explanation of the action of these drugs is that they somehow injure perception in the direction of the basic disease process or personality set-up already present.' The data of the present investigation do little more than add to the anecdotal evidence for such a viewpoint; most explicit, perhaps, was the comment by one of the subjects that 'as a result of nitrous oxide you become what you are, only more so'.

The results described must needs leave also unsolved the problem of just how far drug-induced and clinical varieties of abnormal behaviour can ever be said to resemble each other, that is, how far they conform to similar laws in their development, organization and extinction. But even if further investigation were to show that the resemblance is merely superficial, the use of drugs may still have methodological convenience. As Russell (1950) has pointed out, one of the most urgent requirements for progress in the field of experimental psychopathology is the development of appropriate measuring instruments for the various behaviour manifestations themselves. By enabling overtly similar manifestations to be produced artificially in the laboratory, drugs are an obvious aid to such research.

I am grateful to Prof. F. R. Winton for much help throughout this research; to Prof. Sir Cyril Burt for valuable suggestions during its earlier and Prof. R. W. Russell during its later stages; to Mr A. Summerfield for advice on statistical and other problems; and to the Medical Research Council for supporting the work by full-time personal grants and grants for expenses.

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THE ROLE OF PAST EXPERIENCE IN APPARENT MOVEMENT: A REVALUATION

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An attempt is made to provide a theoretical framework within which data concerning the effect of stimulus connotations on perceived movement might be evaluated. Jones's & Bruner's (1954) results, and those of our own study, have been examined in this context.

In both cases, it was possible to demonstrate that loading through meaning, to be effective, had to be compatible with generalized past experiences with movement, or (from a structuralist viewpoint) with physiological mechanisms underlying movement perception.

Jones's & Bruner's failure to obtain satisfactory positive results could thus be regarded as due to their underestimation of determinants underlying stroboscopic movement as such. Similarly, successful loading with an adaptation of von Schiller's paradigm could be anticipated, given the fact that equipotential movement alternatives were provided. The extent of the phenomenal results of loading through connotation was assumed to provide a measure of the influence of relevant past experiences.

I. INTRODUCTION

Jones's & Bruner's recent paper (1954) reopens an old controversy, by setting out to 'demonstrate some of the ways in which movement-related expectancies condition the perception of movement in a typical stroboscopic experiment' (p. 157).

The Jones & Bruner paper unfortunately does not contain a review of other efforts to deal with the problem, or of the controversy surrounding it. It is one of the objects of the present paper to fill this gap. This involves not only a summary of experimental work paralleling that of Jones and Bruner, but also an attempt at a systematic statement of the theoretical problem of the role of past experience in apparent movement. In this connexion, an attempt will be made to trace out the specific points of difference and agreement between structurally oriented theorists and contemporary empiricists.

Finally, experimental implications will be concretized by a detailed evaluation of the Jones & Bruner data and of an experiment by the authors. The purpose of this section will be to clarify the problem of hypothesis formulation and testing in the area of meaning and movement.

(i) *Experimental background: the use of 'meaningful' stimulus objects*

A direct demonstration of experiential determination of movement perception would be a genetic study showing that such perception was acquired by the individual in the course of his development, as a result of dealings with the environment. This type of research is precluded by the fact that infants already perceive movement at the earliest ages at which experimentation with them is possible. In fact, it has been demonstrated that apparent movement thresholds are considerably lower for children than they are for adults (Gantenbein, 1952; Meili & Tobler, 1931) suggesting that movement perception is genetically a primary category.

Given the practical impossibility of a direct approach, one is faced with the necessity of experimentally attacking the problem of the role of past experience by way of the movement percept of the adult. This indirect approach has taken two lines: (1) the endeavour to obtain, from protocols, evidence regarding object characteristics in perceived movement; and (2) the demonstration that variations of meaning connotations in the stimulus can influence thresholds or qualities of the movement percept. Since the Jones & Bruner study falls into the second category, the following review will concern itself with experimental literature under this heading.

With regard to object characteristics in movement, it may suffice to note that reports of movement without secondary qualities have been obtained. It has been argued, however, that this 'represents nothing but a bit of interesting observation under fairly specialized conditions' (Neff, 1936, p. 14), and not a fundamental observation. Moreover, the possibility of meaning operating *unconsciously*, as seen by modern empiricists (cf. below), makes this question largely academic.

The first investigator to report the use of meaningful material in apparent movement studies is probably DeSilva (1926), who investigated the role of 'subjective' determinants in movement. Among his findings he specifies that 'in general, an increase in the complexity of the object favors good (stroboscopic) movement. The perception of movement of a solid object is clearer than the perception of movement of a unidimensional object'. Moreover, 'a meaningful, animate pattern gives a clearer perception of movement than an unmeaningful inanimate pattern' (p. 500). DeSilva's findings are not unambiguous, since no indication is provided as to whether more differentiated structure or greater meaning connotation underlies the decreased threshold he reports.

Another investigator using meaningful figures in apparent movement experiments was Blug (1932). The stimulus figures were exposed simultaneously (with a tachistoscope) and movement was obtained by means of rapid brightness changes, as originally suggested by Engel (1928). In meaningful material, Blug reported not only the usual centrifugal and centripetal movements in the field, but 'also apparent movement of the exposed objects. A horse appears to move in the direction suggested in the picture; a bird seems to beat its wings, etc.... The apparent movement is especially pronounced, when its direction coincides with that of the movement elicited by the brightness change' (p. 304). Meaning connotations are reported not only to improve the movement perceived with brightness change, but to enable the perception of movement directions at variance with those observed in response to the brightness alteration *per se*.

A similar type of observation, under different experimental conditions, was made by Frieda Fuchs (1928), who studied projected afterimages of movement. She found that 'with the projection of the afterimage of motion unto...solid objects, there developed a great degree of dependence (of the perceived movement) on past experiences with these objects. The objects appeared to execute, under the influence of the afterimage, movements observed in them in the past. Thus a ball appeared to rotate on its visible axis, *irrespective of the direction of the objective movement from which the afterimage originated*' (pp. 314–15) (*Italics ours*).

Krolik's (1935) paper comprises an exhaustive series of experiments utilizing meaningful figures. The procedure used was that of induced movement, a phenomenon systematically investigated by Duncker (1929) and Oppenheimer (1935). Induced movement has been defined as resulting from the alteration of the distance between two stimuli in an otherwise homogeneous field. It is 'induced' because the movement is allocated irrespective of which of the stimuli is displaced.

Duncker had found that given two figures, one of which does not enclose the other and neither of which is preferentially fixated, movement tends to be equally distributed to the two figures. These would either be perceived as moving away from each other equally, or as alternating on a 50–50 basis. Krolik substituted for Duncker's geometrical figures, slides containing drawings of objects customarily experienced as mobile (car, balloon, ship, train, hand cart, elevator) or stationary in past experience (house, barn, log, beacon). 'With an alteration of the distance between a "house" and a "car",' inquired Krolik, 'will—irrespective of which of these objects were objectively moved under induced movement conditions—the "car" be seen as preferentially moving?' (p. 89). 'If...it should develop that the movement favored by past experience is preferred, we shall be inclined to regard this as a consequence of experience with relevant movement' (p. 53).

Krolik found, under almost all conditions, allocation of movement unilaterally, in favour of the object experienced in the past as moving in the relevant direction. Not only did the 'car' drive away from the stationary 'house', and the 'ship' pass the 'lighthouse', but the 'train' moved horizontally away from the ascending 'balloon', and the 'push cart' behaved similarly with respect to the 'elevator'. Great care was taken to control all possible structural determinants in these conditions, with no significant alteration of obtained results. Even with knowledge, and in a semi-darkened room, it was found almost impossible to offset the effect (p. 83).

Although admitting the positive character of these results, Krolik explains them on the basis of the *principle of enclosure* operating through past experience. His argument will be considered in some detail below.

It remains to touch on an impressive demonstration recently originated by Adelbert Ames, Jr., which has been dealt with in the literature as the 'rotating trapezoid window' (Ames, 1951; Ittelson, 1952; Kilpatrick & Ittelson, 1951). In this demonstration, a flat trapezoid painted to resemble a rectangular window in perspective, appears to oscillate when rotating. The phenomenon can be elicited with a non-painted trapezoid but the impressiveness of the oscillation is greatly diminished, and its radius decreased. The illusion is generally interpreted as the result of cumulative past experiences with rectangles in general, and windows in particular. The weighted average of such experiences is said to make for an unconscious choice (among the possible interpretations of the retinal outline corresponding to the demonstration) of the 'window' interpretation, which requires oscillatory movement. The meaning 'window' thus determines the nature of the perceived movement.

All of the above studies have reported successful modification of movement experiences through the substitution of meaningful material for geometrical figures. DeSilva improved the quality of stroboscopic movement percepts; Blug obtained specific meaningful movements in lieu of brightness-induced gross movement experiences; Fuchs also obtained specific movements through the combination of solid objects with simple movement afterimages; Krolik determined the adjudication of induced movement; Ames reversed the perceived movement direction of a rotating surface. It remains to consider what inferences may legitimately be drawn from these positive findings.

(ii) *Theoretical background*: *the empiricist position*

Controversy concerning the processes underlying movement perception has largely centred around *phi*, the movement perceived with stroboscopic succession. This phenomenon has been regarded by investigators of the gestalt school as a fundamental source endeavour to obtain, from protocols, evidence regarding object characteristics in perceived movement; and (2) the demonstration that variations of meaning connotations in the stimulus can influence thresholds or qualities of the movement percept. Since the Jones & Bruner study falls into the second category, the following review will concern itself with experimental literature under this heading.

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(ii) *Theoretical background: the empiricist position*

Controversy concerning the processes underlying movement perception has largely centred around *phi*, the movement perceived with stroboscopic succession. This phenomenon has been regarded by investigators of the gestalt school as a fundamental source of information concerning perceptual processes. Explanations of *phi* in this framework have been couched in physiological terms. The principal hypotheses are probably Wertheimer's (1925) 'short circuit' theory, Koehler's (1923) 'Stromfaedensaeule' displacement hypothesis, and Koffka's (1935, p. 280 ff.) discussion of process fusion. Criticisms of this emphasis as a whole have largely centred around the fact that apparent movement is subject to influence through attitudinal factors, and that hence it could not be an elementary physiological product. (Cf. DeSilva, 1926; Linke, 1929; Higginson, 1926; Neuhaus, 1930; Peterman, 1932; Pieron 1952.)

Gestalt psychologists have not only acknowledged the effectiveness of attitude in experimentation under the heading of 'attitudinal set' (Cf. Wertheimer, 1925, pp. 44–52) but have even specified it as a determinant in marginal situations (e.g. von Schiller, 1933). This same position has been taken in relation to past experience. Thus Wertheimer makes the parenthetical statement that 'it is certainly correct that past experience can influence conditions of stroboscopic perception' (p. 79). Metzger more recently indicates that 'experience has been assured a role as an independant force'. However, he adds: 'It can only manifest itself in conjunction with other forces, at times very powerful forces, which have their origin partly in Gestalt-relationships and partly in the condition and attitude of the observer'. Furthermore, 'without the existence and operation of (gestalt) laws experience could never be accumulated nor applied' (1936, p. 154).

In view of this position of gestalt theories, and since no empiricist would deny the physiological substratum of perception, the fundamental question has been reduced to one of relative primacy or importance of learning processes and physiological mechanisms, instead of one of exclusive determination. The difficulty of assigning priority is pointed up in Krolik's discussion of his experimental results.

As will be recalled, Krolik found that movement (in induced movement situations) was allocated in line with meaning connotations of the two subjects. He concluded, however, that these apparent effects of past experience are actually the result of the operation of the 'principle of enclosure'. This is Duncker's structural law for movement allocation which simply states that 'the enclosed object moves more than the enclosing one' (1929, p. 205). Krolik bases his argument (1) on a single empirical finding with a special presentation (the 'enclosing landscape' slide), and (2) on an *ad hoc* theoretical construct, that of the 'apparent' (non-manifest) frame of reference.

In the 'enclosing landscape' condition the two stimulus objects extended over a screen directly in front of the subject. One of these objects was a small 'house' in the centre of the field. The other, consisting of 'movable' components such as clouds, a zeppelin, water, boats and ships (together with a barely perceptible shore line) completely enclosed the 'house'. When the displacement took place it was the house which appeared to move, despite its stationary connotations. From this fact Krolik (pp. 90 ff.) concludes that experience as a determinant is secondary to the structural law of enclosure.

This conclusion, however, does not necessarily follow from the observation. Not only does past experience not ordinarily provide objects travelling at identical speeds (and not relative to each other) but no empiricist would deny the possibility of eliciting movement in stationary stimuli. More important, it can be convincingly argued that in past visual fields objects (figures) have been invariably experienced as moving against stationary backgrounds (ground). The prediction of Krolik's observation could thus be made from empiricistic considerations.

Equally susceptible to rebound is Krolik's argument about 'apparent' frames of reference. Krolik observes that meaningful figures, in contradistinction to geometrical ones, seem supported on invisible surfaces, and appear to be surrounded by non-visible 'atmosphere'. He concludes that each object has its own frame, with reference to which it moves. These frames are not provided in the presentation, but are psychological (unconscious) addenda. Movements according to experience thus occur to 'facilitate the reproductive formation of apparent...enclosure' (p. 97). In other words, 'the effect of past experience in our experiment can be easily categorized under the autochthonous laws, as these were formulated by Duncker in his law of "allocation of movement" for structural movement constellations: "phenomenal movement is the displacement in a natural frame of reference"' (p. 100).

The observation concerning invisible support and apparent atmosphere can, however, simply be regarded as meaning that, for past experience to operate (in the shape of object connotations) stimuli have to be susceptible to interpretation (at some level) as concrete objects. Such interpretations would involve unconscious size-distance assumptions.

The Ames Demonstrations, one of which has been discussed above, are in part designed to make the point that the "thatness" of an object is an experiential product, and its 'thereness' (hence, its movement) is an inseparable corollary of it. From this point of view it can be successfully argued that without the assignment of the meanings 'car', 'house', etc., the different apparent distances of these objects could never result. Since Krolik does not provide for the origin of his 'frames', his conclusion is not an alternative to such an argument.

The above would certainly be more in accord with the bulk of Krolik's results, and his original hypotheses, than the *ad hoc* explanation he presents.

We have here referred ourselves to generalized past experiences, such as that objects move relative to backgrounds and have implicit sizes and distances. Experiential effects of this kind were not allowed for by nineteenth-century empiricists of the associationist school. Contemporary empiricism, however, is not couched in terms of isolated object connotations, but rather of weighted averages of experiential sediments of all kinds, acting inseparably in the interpretation of intrinsically meaningless receptor events (Cantril, 1950; Ittelson & Cantril, 1954; Kilpatrick, 1952*a*). (These sources, representing the 'transactional' approach to perceptual theory, are cited as illustrative of the empiricist position, not as its exclusive exponents. Other writers have taken compatible positions with regard to perception in general and apparent movement in particular. Among the latter are Gemelli and his students, as well as the Innsbruck group—Erismann and Lehmann. Space limitations preclude a discussion of the specific points of view of these investigators.) Past experience in this context would not only operate on a 'house-is-stationary' and 'car-is-mobile' level, but would make for the interpretation of 'house' and 'car' in the first place, and of the relevant spatial and ordinal characteristics.

Stroboscopic presentations would be expected to 'move' because of cumulative experiences with all kinds of sequences. That is to say, the infant's experiences from his earliest days with rapid succession of sensory data in successive receptor locations have responded to the interpretation 'motion'. This—it would be postulated—has created a very strongly weighted unconscious translation of ordinal events into 'movement'. Given the basic character of this unconscious translation, one would expect it to outweigh object connotations, when the two orders of experience are in conflict (as they are not in every day experience).* The prediction, in this situation, would be the reverse of what an associationist might have expected, and in line with 'gestalt' predictions in the same case. The only point of difference between the contemporary empiricist and the structuralist would concern the 'ordinal' determinant, which the latter would regard as a structural law of physiological origin, and the former, as an experience-derived 'assnmption'. (The same would hold true of the so-called 'autochthonous' factors in space perception.) The possibility remains that this issue, given its generality, may eventually disintegrate into a verbal distinction.

Since 'gestalt' theorists admit the feasibility of determining movement percepts by object connotations, when so-called 'structural' factors are neutral, and empiricists do not make further claims for object connotations, experimentation in this area would not be critical for the empiricist-nativist controversy. The only type of critical experiment would be a direct (genetic) approach, which is not practicable (cf. p. 195, above).

The possibility that experimentation with meaningful material in movement may not be decisive for the isolation of the role of experience does not invalidate such work. For structurally oriented investigators, this type of experimentation may be regarded as gauging the extent of the potential effectiveness of experience (as a perceptual determinant) when set against other (physiological) determinants. For the empiricist, the relative weights assigned to different experiences in perceptual organization would be under investigation.

The following will attempt to translate the empiricist formulation into experimental terms through an evaluation of data. The emphasis will be on the qualifications a realistic working hypothesis in this area should meet, in accordance with the above.

(iii) *Limitations to the experimental demonstration of the role of experiential connotations*

It has been implied above that in line with both nativistic and contemporary empiricist theory, the stroboscopic sequence $a1$–$b2$ would constitute a mandatory condition (if presented above *phi* threshold) for the perception of movement from a to b, and no other. The introduction of content suggesting a reverse movement would not be expected to offset this direction, although it could be expected to raise the movement threshold.

This type of presentation would involve, for the empiricist, a conflict between heavily weighted assumptions of long-term origin (as to movement in general) and relatively weak assumptions activated by meaning connotation.

In order for movement to occur in line with specific presentational content, the latter would have to suggest a sequence compatible with what has been learned regarding movement in general. The conflict, in order to be resolved in favour of meaningful connotations, has to be between two or more empirically plausible potential movements. The role of content in experiments featuring stroboscopic movement, in other words, has to be that of loading one of the potential movement alternatives (as more in line with past experience than another), the prediction being that such loading would make for preferential movement in the suggested direction. Content would play the part of reinforcing one of the possible resolutions of a choice situation.

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* This occurs, for example, in the 'rotating trapezoid window' with a cross-bar attached. The bar appears to 'cut through' or 'bend around' at apparent turning points despite object connotations to the contrary, in order to allow for meaningful sequence. Interestingly enough, this non-object behaviour is subject to experimental control through variations of object connotations. Interpretations of 'rubber' or 'steel' can determine 'bending', or 'cutting' (Kilpatrick, 1952*b*).

(iv) *The Jones and Bruner study*

In the Jones & Bruner experiment, where 'the relationship of stimulus context to postulated general expectancies' was under investigation ('stimulus context' being defined as the 'experimentally manipulated stimulus framework in which movement occurs' and 'expectancies' as 'the pre-established network of hypotheses about movement in the subject') the *primary* context would be the extent and direction of movement with neutral (equally weighted) figures, and the *basic* 'expectancy' that of *a* to *b* movement of an *a*1, *b*2 presentation. Degree of departure from this basic movement would represent success of loading through meaningful connotation.

It may be relevant to examine the experimental data, as reported, for degree of success in loading.

The first of the Jones & Bruner experiments contrasts the apparent movements of a meaningful figure (stick man) and a meaningless one (nonsense object) relative to each other. The stroboscopic presentation is a three-stage one, with each object ending at the point of departure of the other. Differential movement favouring the 'stick man' is predicted. Such differential movement is, also, observed—at first—but 'on the fifth trial...no longer obtains. Eight of the subjects saw the (two) moving at an equal speed, and eight of the remaining ten saw the nonsense object as moving faster' (Jones & Bruner, 1954, p. 159).

Jones & Bruner dismiss the reversal with a reference to 'satiation', and conclude that 'a man-in-motion has a higher subjective probability value than a thing-in-motion' (p. 159). The following 'hypothesis' is regarded as 'confirmed': 'When two objects cover an identical distance objectively, the object which has more frequently been seen in motion will be seen initially as moving faster and farther' (p. 165).

It is a matter of possible dispute whether a restatement in general terms of a finding under specific conditions is methodologically justified. However, if it were, one might equally well conclude that figures which have not been observed to move in terms of experience will in general tend to move faster and farther after a few initial trials, requisite for structuring of the percept.

In point of actual fact, preferential movement of a figure denoting a 'moving' object as against one that does not carry such denotations has been obtained (with relative displacement) under induced movement conditions. The effect there reported is unambiguous, and the 'non-moving' figure is seen as stationary throughout (Krolik, 1935, cf. above p. 197).

The Jones & Bruner presentation: (1) Presupposes that stroboscopic intervals would not *per force* elicit the same degree of movement in both figures, for which no rationale is offered. The movements called for would not *a priori* have to be differential since they are not mutually exclusive (cf. Higginson, 1926, who has obtained different movements over the same space). *Since there is no forced choice or conflict, and the presentation is one which permits the perception of a configuration involving two discrete movements, the hypothesis is far from convincing.* (2) A greater difficulty in identification of the nonsense figure may underlie the initial reports. (3) Any remaining possible effects in favour of the 'stick man' are neutralized by having this figure larger, which would tend to make for slower apparent movement (Brown, 1931). Hence, probably, the reversal.

The utility of a 'nonsense figure', of course, rests on the presupposition of its meaninglessness. Whether no meanings are actually assigned to such figures is open to question. In fact, Jones & Bruner's 'nonsense figure' looks almost human, and its presentation in conjunction with the 'stick man' may reinforce this significance.

Jones & Bruner's second experiment compares the subjective goodness of movement in meaningful and meaningless presentations, after one stage (a middle terminal) is omitted in both. It is reported that the break is felt more in the control condition. However, since fully half the controls did not report any change, and 21% of the experimental group did, a statistical test of the difference appears desirable. This is not provided, because 'inadvertently the original data for this experiment were destroyed' (Jones & Bruner, 1954, p. 160). Therefore, although the paradigm is an interesting one, the conclusions that 'the more probable and practised the movement, the more adequately will the movement experience maintain itself under sub-optimal conditions' (p. 165) may require further substantiation. Again, the basic nature of apparent movement perception as such may make the obtaining of a significant difference here a difficult task.

The third of the four experiments described in the Jones & Bruner paper featured a diagonal line, and two sets of three circles successively exposed on it in a downward direction. The circles contained an inset arc, in three different positions (left, upper right and lower right) in one case, and one constant location (left) in the other. The differential quality of the movement reported in these two presentations (predominantly 'rolling' as against a modal 'skidding') is interpreted as a function of expectancy, with the circle being supposedly interpreted by the observer as a 'ball'. Unfortunately, standard structural explanations for movement direction can be brought to bear on the difference in movement quality, such differences being generously represented in the literature. The 'ball' interpretation is hypothetical, and the experiment therefore is far from critical.

The fourth and last of the reported conditions sets itself precisely the task outlined above as the realistic test of experience effects. 'The general hypothesis is that where there is an option in the path of apparent movement, that path of movement will occur, other things being equal, that most conforms to an established expectancy' (Jones & Bruner, p. 163). The basis for the presentation used in this experiment is supplied by Ternus, who found that dots successively exposed as (1) spatially separated, (2) together, and (3) separated, could be optionally seen as 'crossing over' or 'bouncing' off each other. These dots, in the Jones & Bruner condition, were shown in the context of (*a*) 'baseball players' (as baseballs), and (*b*) 'billiard players' (as billiard balls). In the first case, they were expected to 'cross over', and in the second to 'bounce off' each other.

Although the results 'tend to support the prediction' they failed to do this conclusively, i.e. the frequency differences fell short of significance. This failure to obtain fully satisfactory results may be at least partly due to the lack of realism in the presentation, testified to by the fact that Jones & Bruner report that 'if the situation was *not understood at all* (*as was frequently the case with the pool players*) the initial stimulus card was taken from the episcope and shown to the subject' (Jones & Bruner, p. 163). The procedure referred to, incidentally, is open to objection, since it may be regarded as tantamount to suggestion. To assert that experiential factors operate, necessitates evidencing their spontaneous intervention in perceptual processing. No basis would otherwise be provided for drawing a line between effects of past experience and experimentally induced set.

The above considerations serve to place limitations on the Jones & Bruner data as adequate demonstrations of the role of meaning or the effect of past experience. They also may illustrate some of the difficulties in this type of experimentation, and the delicacy of the experimenter's task.

II. CHOICE AS A PARADIGM: AN EXPERIMENTAL ILLUSTRATION

The experiment reported below is derived from a rationale similar to that of the fourth Jones & Bruner experiment, and analogous to Krolik's (1935) induced movement study. The object was that of investigating the effect of meaning in a situation where adjudication of movement is not determined, i.e. where a choice is open.

The experimental framework, identical to that used by von Schiller (1933) in his study of assimilation in apparent movement, is analogous to Linke's 'double cross' presentation. This is a stroboscopic presentation consisting of a vertical and diagonal cross, flashing alternately in the same position. A single cross is seen to rotate, under these conditions, in one of two equipotential directions (Linke, 1929, pp. 326–7).

Von Schiller posed himself the question: 'Under what conditions will one movement percept (direction, kind of movement) be preferred out of two geometrically equivalent possibilities in the objectively given stimulus constellation? Generally formulated: What determines the resolution of a set of alternative stroboscopic movements?' (p. 181).

As his experimental condition, von Schiller used an arrangement in which the first figure was flashed in the centre of the presentation, and the *b* figures (simultaneously) around it. Variations were introduced by manipulating the similarity of one or more of the *b* figures to the *a* figure, altering *a*–*b* distances selectively, or changing the total pattern.

Von Schiller found that in general the *a* figure would tend to divide and radiate *to any and all b figures*, a tendency which he called 'tendency to total assimilation'. However, *preferential movements* sometimes occurred, notably over shortest distances and to the most similar figures. Selective attention to parts of the presentation made for preferential movement favouring the member attended to.

The following exploratory experiment, carried out by the authors,* introduced meaningful connotations into von Schiller's paradigm. An arrangement of three figures was flashed in *b a b* sequence, with the middle one preceding the outside figures, which were flashed simultaneously.

Flashes had a duration in the order of 50 msec. The intervals used were 40 and 70 msec. Intensity was systematically varied, with an arbitrary optimum value of 11 foot-candles being used for the bulk of observations.

The stimulus objects were *c.* $1\frac{1}{2}$ in. long, and were flashed $1\frac{1}{2}$ in. apart, 7 ft. from the observer. Observations were carried out binocularly, in a totally darkened room, with the observers dark adapted. A total of eight observers was used.

The apparatus was built around a mechanical timing device operating through a set of relays, and a series of variable transformers. The viewing unit consisted of a box divided into three compartments by tin partitions, each containing two $7\frac{1}{2}$-watt filament bulbs facing a pane of diffusion glass. Between these boxes and the coated glass fronting the entire unit, a slit allowed for the insertion of a photographic slide containing (white on black) figures.

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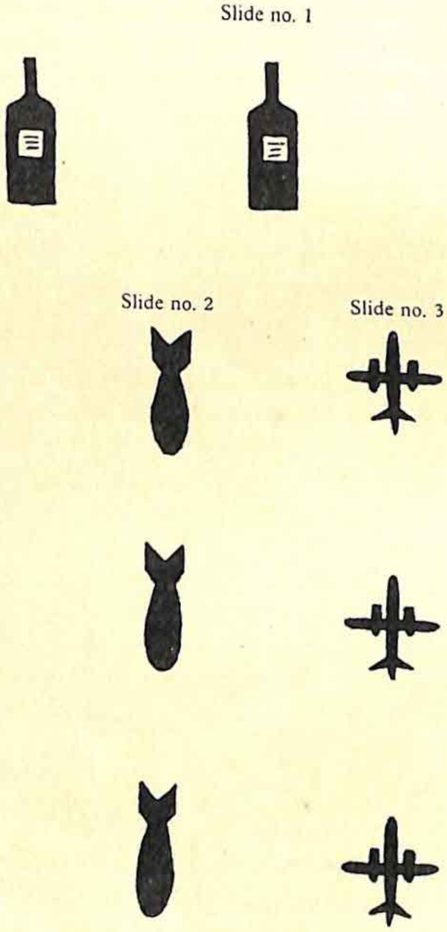

Slide no. 1

Slide no. 2

Slide no. 3

Fig. 1.

Three slides were used for the purposes of the experiment. One of these was a control slide, and consisted of a series of three bottles (Fig. 1). The rationale for this presentation was that meaning-connotations of a bottle are relatively neutral as regards movement direction. It was further considered that neutral meaningful objects carried a methodological advantage over abstract or nonsense figures, in that the possibility of arbitrarily assigned meanings is minimized, and the situation is more strictly analogous to the experimental one.

Of the two experimental slides, which were flashed vertically (in order to be comparable, and serve as additional control to each other), one contained solid outline drawings of three bombs (pointed downward), and the other featured three two-engine airplanes (nose upward). In order to preclude, in so far as possible, the operation of structural factors, the bottom of the bombs was rounded, and the wings of the planes were located toward the centre of their bodies. Although this has not entirely eliminated the possibility of structural determinants operating, the probability is considerably reduced. Given the fact that all three presentations are symmetrical, that distances are equivalent and the three figures identical, von Schiller's results would permit the prediction of movement outward from the centre unless meaning intervened to the contrary. It was postulated that, in the experimental slides, the direction in which the objects represented would move according to past experience would be that in which preferential movement (of the kind elicited by shortest distance or greater similarity) would be seen by observers.

III. RESULTS

The results, for the eight observers used, have been summarized in Table 1. As will be noted, five of the observers saw symmetrical movement in the control slide. For these observers, the middle 'bottle' was stationary, while the other two appeared to move away from it. Two of these observers saw occasional preferential movement, mostly toward the right, where the experimenter's red light was located.

Table 1. *Experimental results*

Number of subjects reporting each observation (total $N = 8$).

Types of observation	Slide 1 ('Bottle' presentation)	Slide 2 ('Bomb' presentation)	Slide 3 ('Airplane' presentation)
Movement:			
(1) Symmetrical (bilateral)	5	1*	0
(2) Unilateral	0	6	5
(*a*) Exclusively	—	4	4
(*b*) Preferentially	—	2	1
Direction: Up	—	0	5
Down	—	6	0
(3) Other movement:	2	0	1*
Total movement	7	7	6
Total succession	1	1	2
Total subjects	8	8	8

* No recognition. Figure seen as geometrical or different.

Of the miscellaneous three (out of eight) observers, one saw the bottles 'wiggle' on the spot, another saw them execute complex three-dimensional loops, while the third reported two bottles, flashing alternately. The only significant difference between the modal response elicited by the control slide, and von Schiller's data is the fact that it is not the *a* bottle which moves to the *b* bottles, but the latter which move away from the centre. (This is the kind of movement described by Wertheimer (1925, pp. 30–4) as 'dual partial movement'.) This may perhaps be interpreted as a function of the connotation 'bottle', which makes it difficult for the figure to split and move in two opposite directions.

Slide no. 2 (the 'bomb' presentation) elicited only one set of 'parting movement' responses. These came from an observer who did not recognize the 'bombs', but saw 'three vertical lines', which appeared to part vertically. The other six observers who saw movement in this presentation, reported preferential movement down. Two of these saw three bombs falling, while the others saw the bottom two bombs moving down, with the top one sometimes stationary, and sometimes 'joining in'. Occasionally only one bomb was reported, which was seen as moving from top to bottom. The two remaining observers who saw movement reported downward movement at higher brightnesses, and parting movement (up and down) at lower intensities.

The picture is closely analogous for the 'airplane' presentation (slide no. 3). Of the four observers who saw movement exclusively upward, two reported all three airplanes as participating, while the other two noted movement almost exclusively by the top plane. One observer saw movement predominantly upward at higher brightnesses, and parting at lower intensity values. The remaining observer who saw movement reported 'three ballerinas, jumping upward on their toes'.

These data clearly show that even with a marked lack of realism in the presentation (which in the case of the airplane is partly due to the effort to achieve structural neutrality) and the sketchy character of the objects, meaningfulness in terms of past experience markedly contributed to the determination of movement direction. Moreover, the effect was more striking than could be predicted, since the tail end figure was at times seen to move in a direction opposite to that in which it would move with neutral objects, such as those used by von Schiller. Thus the perceived movement is more preferential than could be anticipated.

An interesting difference in reported movement direction at times manifested itself with varying brightness, increased intensity showing itself as favouring the determining role of meaning. At higher intensity movement was also reported as faster, which may have a bearing on the fact. (The problem of apparent velocity in stroboscopic presentation of different brightness still awaits systematic investigation.)

The above experiment may be taken as demonstrating the utility of von Schiller's paradigm for investigations of the effect of meaning on direction and quality of perceived movement.

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THE STATISTICAL THEORY OF FIGURAL AFTER-EFFECTS AND ACUITY

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Osgood & Heyer explain the shift of a contour by the prolonged inspection of another in the vicinity by assuming a spread of excitation in the cortex, associated with each contour. This spread of excitation is normally distributed. The contour itself is 'perceived' at the peak of the distribution. The shift in the second contour occurs because of an area of depressed excitability, left by the first, which will tend to skew the second distribution of excitation, and its peak, away from the locus of the first. It is argued that if the two distributions are to subtract from each other successively, they will add up to a single peak if placed on the cortex simultaneously. Thus any interaction will occur only if the two contours are below the threshold of separation and any shift will be a mere fraction of this threshold.

Various amendments are considered which might enable the theory to evade this consequence. The neurophysiological basis (Marshall & Talbot) for Osgood & Heyer's theory is also questioned. A hypothesis to explain figural after-effects and the after-effects of seen movement is briefly suggested.

I. INTRODUCTION

When a visual contour has been fixated for a minute or two, other contours placed in the vicinity are phenomenally shifted away from the locus of the first contour. This shift is not largest the nearer to each other the two contours are; the maximal after-effect is secured when the two lines lie at a small distance from each other. This fact is known as the distance paradox. Though workers such as Gibson (1933) obtained such effects, it remained for Köhler & Wallach (1944) to investigate them thoroughly and to clarify their genesis. Köhler & Wallach's empirical work on figural after-effects, as these illusions are called, has largely been confirmed and extended; their theoretical interpretation has been hotly disputed. The neurophysiological assumptions peculiar to the theory have been amply criticized, both experimentally and theoretically, by Lashley, Chow & Semmes (1951). Osgood & Heyer (1951) have made an attempt to construct an hypothesis consistent with what they take to be the facts of neurophysiology (that is, the speculations of Marshall & Talbot, 1942). This schema, it is claimed, can account for the facts observed equally as well as those of Köhler & Wallach. Like many theories in psychology, such as Hull's (1952) and Hebb's (1949), it has implications which are never brought out by the author and which render the theory self-defeating. Absurd consequences are generated and pass unnoticed. However, to overlook these is all too easy, and the author will be very sorry if any criticisms he makes obscure the real merits, the ingenuity and the simplicity of Osgood & Heyer's theory.

II. CRITICISM OF OSGOOD AND HEYER

Any theory of figural after-effects has, as its main task, something which appears almost too simple—to explain why one line, after being exhibited for some time, begins to push all others away. Osgood & Heyer picture this in terms of a mechanism somewhat as follows.

The authors assume 'that the apparent localization of a contour in subjective space coincides with the location of maximal excitation in area 17' (Osgood, 1953, p. 236).

'Drawing directly on the work of Marshall & Talbot (1942), we may assume that the representation of a contour in the projection cortex is a normal distribution of excitation symmetrical about its own axes transversely and extending as a "ridge" throughout the longitudinal extent of the contour'. The shift in excitation, representing another contour placed close to the locus of a previous one, occurs because 'under constant fixation of a figure, the cells in area 17 mediating the on-off activity will become differentially adapted as negatively accelerated functions of (*a*) the rate of their excitation and (*b*) the time through which they are excited' (Osgood, 1953, p. 238). Hence 'the locus of maximal rate of fire in the distribution representing a T-contour in subjective space can be shifted because of the differentially adapted region in which it falls' (*ibid.* p. 238). This would explain a shift. Unfortunately for the theory such a shift could hardly be seen.

The adaptation is held to take place in the previously excited area. Hence, only a contour falling within this area would show any shift. But consider the two contours displayed simultaneously. Their distributions of excitation would summate if their areas of excitation overlapped. Now it is anybody's guess how far the two distributions must be merged before they are treated as one and the same distribution, but there must come a point somewhere before the combined distribution loses its two humps and summates into one very large one. Approximately the latest point, given an equality of the two distributions, at which discrimination can occur, is when the two points, which are half the height of the peak of the two distributions taken separately, are brought into coincidence, given a normal distribution. It will be the distance between the two peaks just before this occurs that would give such a system, at its best, a threshold for the discrimination of two adjacent lines. These considerations carry implications about the figural after-effects to be predicted from this theory.

(1) By inspection of Osgood's figures, the optimum distance for a maximum effect (the distance paradox) is well within the threshold, as calculated above, for the discrimination of two adjacent lines.

(2) Needless to say, the maximum displacement occurring on this theory (again taking Osgood's diagrams), is very much smaller than the optimum distance for a maximum effect. This means that it is much smaller than the threshold for the discrimination of two adjacent lines. It becomes difficult to understand how such displacements could be reported and to be seen to be decreasing by the subjects. It could be said that the shifts were large because thresholds were high on account of the conditions of the experiment. But however high they were and however poor the conditions of the experiment, the displacements could still only be below the thresholds for angular separation there and then. The trouble with the theory is not that it necessarily predicts too small an after-effect but that it must always predict an effect which is presumably too small to be seen.

(The estimate of threshold here is based on that for angular separation. Yet, with the methods of estimating size and distance typically used in the experiments on figural after-effects, the thresholds for angular separation were almost certainly smaller. It might be held that the displacements in the figural after-effect experiments could be smaller than the minimum separable and, yet, be visible. The threshold for the visual perception of the smallest misalinement is quoted by Polyak (1941) to be from 8″ to 2·5″ of arc and even less. But this is a different judgement; in the typical after-effect experiment, Weber's law should give us a far better approximation to what the difference limen was. Nevertheless, however we estimate acuity in this situation, the fact remains that Hammer (1949) reports a figural after-effect of 2′ where the expected minimum separable is of the order of 1′.)

Is there defence to this for Osgood & Meyer? The force of the criticism rests on a connexion between thresholds for separation and shifts of peak on a 'statistical theory'. One contour can only displace another if their distributions of excitation overlap to a large degree. But if they do overlap to any extent, the distance between them must be subthreshold (or very near threshold). Further, the amount of displacement possible on this theory must always be a fraction of the distance between the contours. Thus, any displacement should be too small to be detected by the subject (or at any rate very much below his threshold for the separation of two contours).

It might be argued that the two distributions of excitation, representing the two contours, do not summate though they overlap and, therefore, we cannot draw any conclusions concerning thresholds from the theory. But clearly, unless each unit of excitation carries some sort of identification mark saying to which contour it belongs, effective summation must occur. If the units of excitation are identical, two peaks of excitation superimposed must add up to one. If there are various types of excitation, this is an important assumption which Osgood & Heyer ought, at least, to have mentioned. It would, however, weaken some of their claims to physiological orthodoxy and, with it, some of their advantage over Köhler & Wallach.

However, any assumptions to yield such properties would complicate the theory immensely. If there are types of excitation which do not interact when simultaneously present, why should they interfere when they occur in succession? Nevertheless, it might be thought that Osgood & Heyer could not have contemplated summation of two overlapping processes to produce the appearance of one process in between and that there must be some easy, but implicit, assumption which would avoid the difficulty. Unfortunately, Osgood uses this very idea of summation (1953, p. 245) to explain stroboscopic movement. 'We already know that smooth, continuous apparent movement can be obtained from discrete, disparate stimulation when they fell within the same region over which lateral contour effects are obtainable. This suggests the hypothesis that apparent movement arises when two contours interact when their excitation peaks are mutually shifted. Since stimulus (*a*) precedes stimulus (*b*) by a brief interval, it follows that the distribution representing (*a*) will be in the process of decay (decreasing rate of discharge) while that representing (*b*) is in the process of development (increasing rate of discharge). The necessary condition for smooth, continuous apparent movement—optimal movement—would be that in which summated distributions produce a single peak that *continuously* shifts from (*a*) to (*b*).... The arrow indicates the shifting position of the single perceived contour'.

(Curiously enough, the lateral contour effects which suggest these summations of excitation to Osgood are the ones described by Fry & Bartley. In his own words (also p. 245) 'it has been shown that the presence of one contour may inhibit the formation of another with 4° *distance*.') Nevertheless, here we have a passage which shows clearly that Osgood contemplates a summation of two separate excitatory processes occurring simultaneously to produce the 'perception' of some intermediate contour. It is true that, when this summation is occurring centrally, the peripheral conditions are not the same as there are not two lines impinging on the retina simultaneously. It could be claimed that, when two contours are simultaneously displayed, the distributions are narrowed by some means (some sort of mutual inhibition) so that they do not overlap. This argument would increase the radius of action of the after-effect in relation to the threshold, and break the dependence on which this criticism is based. But even this addition to Osgood and his postulates can hardly be a way out because of the facts of self-satiation. How could two non-overlapping excitations act on each other in such a way as to produce a shift? Osgood & Heyer would have to make *ad hoc* assumptions of some complexity to adopt this line of defence.

Superficially, the most attractive way of breaking the dependence of threshold on amount of after-effect is by altering the shape of the distributions of excitations which Osgood & Heyer posit. Oddly enough, this ancient panacea which has, in its time, revived a great many theories just as they were threatened with demise does not seem to help a great deal here. We should have to invent a curve giving a separation threshold of 1′ 15″ and a figural after-effect of 2′ when the two peaks were 22′ apart, of such a shape as to yield a distance paradox. This would be a good problem for producing a nervous breakdown in a calculating machine!

These two figures for after-effect are taken from Hammer, whose work Osgood also quotes (1953, p. 241). The figure for acuity is corrected for angular distance of 1° 30′ from the ideal foveal centre from Wertheim's curve, as reproduced by Polyak, to make a generous allowance for the fact that Hammer was working roughly this amount out to the periphery (Polyak, 1941, fig. 99).

A related way of escape, which would alter the theory still further, is that the two curves which are interacting are unlike both in shape and extent. Two possibilities, which are not mutually exclusive, are that, during eye movements, a peak of excitation travels hither and thither over the surface of the cortex leaving a band of inhibition very much broader than its own extent at any moment, and (or) that there is an area of inhibition much larger than, and surrounding, the strip of excitation and which persists longer. The second possibility has, indeed, much evidence to recommend it. However, it seems that both suggestions founder on the same objection. The curve of inhibition, though broader than that of the superimposed excitation, cannot be higher. (Otherwise, the second curve would be completely obliterated in some positions.) This means to say that the incline of its shoulders cannot be very steep. On the other hand, even a vertical incline of inhibition could not shift the peak of an excitatory distribution more than half the breadth of the distribution. The less steep the incline, the less the possible shift. Hence, we should expect a larger 'figural after-effect' when the previous distribution was narrow (hence falling away more sharply) than if it was broader. Therefore, although we could expect the after-effect to be at a greater distance than on Osgood & Heyer's original theory, it would necessarily be of an ever smaller magnitude.

III. THE WORK OF MARSHALL AND TALBOT

But is Osgood & Heyer's theory not founded on the work of Marshall & Talbot and, thus, on a sound neurophysiological basis? First, Marshall & Talbot's theory is vague and complicated. The neurophysiological observations they quote are highly ambiguous and consistent with interpretations quite different from those which these authors choose to make. Some of these interpretations are, furthermore, implausible. It is, for instance, difficult to accept, on *a priori* grounds, the notion that excitation from one receptor adds up with that from the next on the cortex. This leads inevitably to a loss of information only some of which can be recovered by a repetition of the message, involving eye-movements, 'memory' and the like.

Secondly, the actual anatomical evidence referred to by Marshall & Talbot is against this notion of summation. Marshall & Talbot's statements regarding the evidence about dendritic overlap in the lateral geniculate body and the cortex are grossly misleading. Summarizing the work of Glees & Le Gros Clark on the lateral geniculate body in the cat and monkey, they say (p. 122) 'Apparently reciprocal overlap is less extensive in the monkey geniculate than in the cat.' Glees (1941, p. 438) expressly states: 'It is interesting to compare these results with the conditions which obtain in the monkey. In the latter it was found that the terminal boutons are extremely few in number, and since it was in no case possible to find more than one bouton related to each geniculate cell, it was concluded that in the lateral geniculate body of this animal there is no overlap of optic fibres'.

That by 'less extensive' they, presumably, do not mean 'absent' is made probable as they go on to assume that such overlap occurs in the human case. 'These unit paths then are related at each synaptic level by reciprocal dendritic overlap of increasing extent, amounting to perhaps 2° at the cortex (Clark, 1941, p. 135).' Again, Le Gros Clark, in the paper to which they refer, makes no statement which can, by any normal processes of reasoning, be interpreted to support this statement, or any part of it.

As this is the work they quote in support for their notion, their contention that there is summation of excitation between separate paths is made unlikely both on *a priori* grounds and by the excellent observations which they misrepresent.

Further, Marshall & Talbot say that (p. 123) 'if paired shocks of weak or moderate intensity are applied to the optic nerve of the nembutalized cat, the second shock produces an enhanced postsynaptic geniculate spike, during the first 10–30 msec. This mixture of supernormality, facilitation, recruitment or summation is succeeded by a longer period of depression or subnormality'. From this they extract a premise very important for their theory, namely, that (p. 127) 'a burst of impulses discharged from the retina would thus be relayed through the geniculate for 10–30 msec., then summation of subnormality would intervene to check the transmission'.

Now, there are three main conclusions to be drawn from the experiment with the two single-spaced electrical pulses. The first is that this supernormality-subnormality cycle occurs in the same way with the same time constants whether there are many pulses or no pulses between the initial and 'testing' pulse. This is Marshall & Talbot's conclusion and it is clearly fallacious unless they endow the initial impulse with special properties not shared by its successors. Otherwise, the pulses succeeding it must either add similar cycles or cancel anything preceding and begin anew. Either of these conclusions is not in harmony with the first one, on which the theory rests. It seems sufficiently obvious that radically different properties are generated by these two last assumptions. Also, we find entities like 'memory' and 'attention' (which themselves must be accounted for in physiological terms) playing a part in explanations which are intended to be physiological: 'The acuity is greatest because the formed edges attract the threshold of attention away from the interspace and background' (p. 146). 'This would allow for subvisible neural action leaving traces in memory' (p. 141).

Further, Marshall & Talbot are very reticent about numerical values and, especially, about the extent of spread of excitation. On p. 135 they suggest tentatively that dendritic overlap could amount to 2° at the cortex. In fig. 6, p. 145, where they are attempting to explain some phenomena connected with the minimum separable, the spread from peak to end of distribution is 3′. Osgood seems to have 4° in mind.

Though a great deal of the explanation of acuity rests on eye movements, there is complete vagueness about the time over which information at the cortex is integrated. Assumptions about the time constants of this seem to be completely *ad hoc* and tailored to fit each particular case.

So far, the criticisms of Marshall & Talbot have mainly been on matters internal to their paper. However, since it was written, more evidence has come forward on this topic of eye movements and their alleged aid to acuity. The work of Ratliff (1952) and Riggs, Ratliff, Cornsweet & Cornsweet (1953) tends to show that acuity is hindered rather than helped by eye movements. 'It has also been demonstrated that the relatively large changes in the position of the eye as a result of the rapid tremor are a hindrance to monocular acuity' (Ratcliff, 1952).

In comparing the two theories, it is difficult to see how Osgood & Heyer's assumption that a contour is seen at the peak of a distribution could fit Marshall & Talbot's scheme. Also Osgood & Heyer reject Köhler & Wallach's three-dimensional projection. Marshall & Talbot do not. 'Alternatively, the "three-dimensional projection" may really exist in the higher level' (p. 158).

Again, Osgood & Heyer deal with contours, Marshall & Talbot with lines.

Thus there does not seem as much to choose between Köhler & Wallach and Osgood & Heyer in the way of physiological respectablity as the latter authors would have us believe.

If this criticism of the statistical theory is valid, it is hoped that Osgood & Heyer will amend it and, again, give us something which is both attractive and simple. Incidentally, there is often a tendency to think that, because one theory on the subject has had doubts cast on it, the other prevailing one must be correct. A criticism of Osgood & Heyer does not necessarily lend support to Köhler & Wallach.

IV. A TENTATIVE HYPOTHESIS

Köhler & Wallach's theory has been thought to be implausible mainly on neurophysiological grounds. However, one of their basic notions seems sound enough. They make displacements in phenomenal space not a shifting of the retinal projection on the cortex but a change in the functional interrelation of this projection. Such a supposition seems intrinsically likely because of both the extent and the distance over which figural after-effects operate. Instead of postulating the flow of direct currents, it is suggested that from each contour printed on the cortex emanates a wave front which travels at a constant speed. When this wave front reaches another contour, a pulse is generated. If then a pulse is also generated when this wave front begins to travel, the time between the two pulses will be proportionate to the distance between the contours. This notion has already been used in a theory of shape recognition (Deutsch, 1955). Here it was assumed that an impulse is propagated from each part of a contour at right angles to its tangent. We can now postulate, in a manner similar to Osgood & Heyer, some sort of refractory state of the conductivity of the tissue where a contour has been generating impulses which it will have done in a particular direction. This will then have the effect of slowing down any travelling wave front; slowing it down, say, in proportion to the directional component which it shares with the wave front previously generated. This would, therefore, lead to overestimation of distance, depending on the orientation relative to the previous contour, of the two new points to be judged. There will also be a narrow fringe round the refractory area which conducts impulses more quickly, perhaps, as a result of a small amount of stimulation or rather as an effect of the abnormal state of the neighbouring tissue. This directional slowing down of two wave fronts (the deviations of whose speed give rise to expansion or contraction, as the case may be, of the interspace) could also, perhaps, explain some of the phenomena of motion and its after-effects, such as the waterfall illusion. The passage of a contour over a given area can be imagined to give rise to an acceleration of the propagation of a wave front in the direction of motion of the contour. The wave front, propagated in the direction reverse to that of the movement of the contour, would tend to be slowed up as it has to pass over an area over which a contour has most recently passed. There would, thus, be a difference in the rate of travel of the wave front propagated backwards and forwards.

When the movement of the contour ceases, this difference in the rates of travel will reverse itself. The forced speeding up in a particular direction will lead to a subsequent refractoriness to transit in the same direction. The wave front propagated in the reverse direction will no longer be passing over a space which has just been occupied by a contour, and the difference in speed of propagation, due to this factor, will swiftly reduce to vanishing point. Thus, impulses will be travelling more swiftly in one direction than in the other as they would be if the contours were actually moving in reverse. This, presumably, would generate at least some of the cues which would arise if the now stationary contours were really moving in the opposite direction.

The difficulty which both previous theories have over the median fissure seems to be more tractable on the present view. If an impulse which has reached the edge is conveyed from one hemisphere to the other sufficiently quickly and continues in the same direction, a functional unity is preserved.

The scheme outlined derives some support from recent neurophysiological observations such as those of Burns (1950, 1951), Burns & Grafstein (1952) and Motokawa (1951).

My thanks are due to Mr N. S. Sutherland for his discussion of the section on Osgood & Heyer and Mr J. Price for a discussion of some of the criticisms of Marshall & Talbot. I am also indebted to Dr Kay for his advice and encouragement at all stages of the preparation of this paper.

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A THEORY OF PERCEPTUAL CONSTANCY

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A terminology is developed for perceptual constancy which permits useful mathematical treatment. This is illustrated by discussing a particular form of constancy for which the mathematics is particularly simple. Other cases which need more elaborate mathematics can be discussed in the same way.

This formulation of the constancy problem enables the empiricist approach to perception to be formulated more concretely. Instead of saying simply *that* learning is involved we are able to go some way towards saying *what* has to be learnt.

It is suggested that constancy can be established by conditioning each element of an 'equivalence class' of states of stimulation to the same group of responses. This idea needs much more elaboration and investigation (Taylor, 1956), and we do not maintain that it is the *only* possible mechanism.

Experimental results on protracted wearing of distorting spectacles are reported and discussed in the light of the above principles.

I. The problem

A central problem of perception theory is that of constancy. We are faced with the fact that objects in the external world stand in a one-many relation to patterns of stimulation of a perceiving organism, and patterns of stimulation stand in a many-one relation to perceptions. Effective perception depends on the result of compounding these relations yielding a relation between objects and perceptions which, at least within certain ranges, will be one-one. About the first of these relations a great deal is known; in fact, the essential features emerge from simple mechanical and optical considerations and from elementary peripheral physiology. On the other hand, extremely little is known about the second relation. One reason for this paucity of knowledge may well be that investigations of constancies in perception have been too *static*. Attention has been focused on investigating the constancies of normal perception, and too little use has been made of situations in which constancies can be destroyed and re-built by experimental manipulation. In the sequel we hope to illustrate this point by analysing a simple but illuminating example of such a procedure. This will lead us to the formulation of a terminology and of certain principles which seem to have a wide applicability in perception theory.

Consider a subject gazing at a scene in which there is no movement. Every shift in the position of his eyes, his head or his whole body will cause the image of the scene in front of him to fall on different parts of the retina and to change in orientation. Yet he sees the scene as stationary and constant in orientation. The mechanism which is responsible for the fact that each of these different patterns of stimulation gives rise to the same perception normally operates in an exceedingly complicated manner and involves a bewilderingly large number of variables, such as retinal stimulation, various forms of proprioceptive stimulation, previous experience of particular objects, and even emotional factors. To gain an insight into the nature of the process we will describe in detail a situation in which the variables can be isolated and described with concrete accuracy. We are not suggesting that all aspects of perception are susceptible to quantitative treatment in the same way. What we claim is that certain aspects which can be described with this kind of precision are capable of elucidating principles which operate universally.

II. A PARTICULAR CONSTANCY

Suppose, now, that our subject is monocular and that he is sitting, with his head erect, in front of a blackboard on which rectangular co-ordinates have been drawn with the x-axis horizontal and the origin in front of the subject's eye. Let $L(\theta)$ stand for a straight line drawn from the origin making an angle of θ with the x-axis, as in Fig. 1. The retinal image of this figure, as inverted by the optical system of the eye, is shown in Fig. 2a, where OX is the image of the x-axis and OL the image of $L(\theta)$. A retinal reference frame is defined by supposing OX to be permanently ruled on the retina, so that it coincides with the image of the x-axis only when the head is in this position. On the other hand, the line OL will always be taken to be the actual image of the line $L(\theta)$. Thus if the subject tilts his head by rotating it about the optic axis through an angle ϕ, the line OX is carried with the retina while OL remains fixed in space. The situation is represented by Fig. 2b, where it will be seen that the image of $L(\theta)$, viz. OL, now makes an angle $(\theta-\phi)$ with the retinal reference line OX.

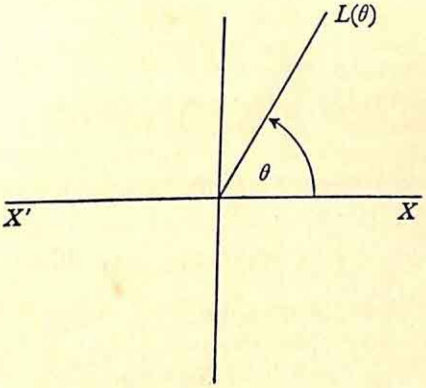

Fig. 1. The objective field.

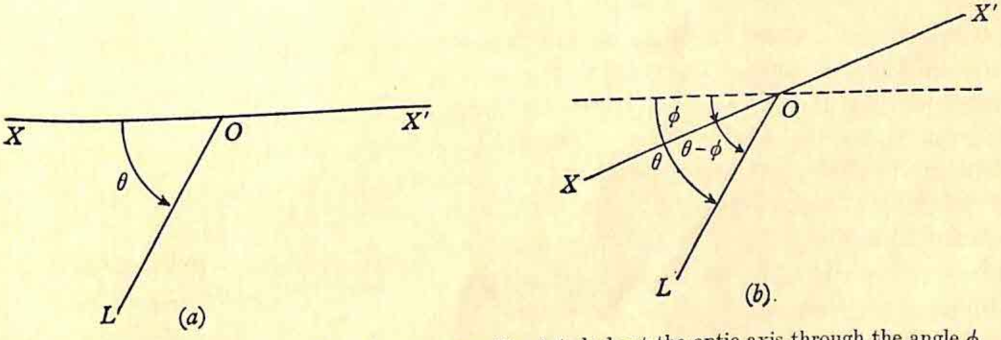

Fig. 2. The retinal field when the head is (a) erect, (b) rotated about the optic axis through the angle ϕ.

As the subject rotates his head in this manner, $L(\theta)$ does not appear to move, although its image is sweeping across a large area of the retina. Quite obviously this constancy cannot be accounted for by retinal variables alone; the proprioception corresponding to the rotation of the head must play a part. The advantage of choosing this simple situation is that we can specify the relevant states of the retinal cells and the proprioceptors by two numbers ψ and ϕ. The symbols (ψ, ϕ) will denote the state of stimulation of the subject when (i) the line OL on the retina makes an angle ψ with OX and (ii) the head is inclined at an angle ϕ. In the situation represented in Fig. 2*b*, (ψ, ϕ) becomes $(\theta-\phi, \phi)$.

We can now write down explicitly the relations referred to in the first paragraph:

$L(\theta)$ can give rise to any one of the stimulus patterns $(\theta-\phi_1, \phi_1)$, $(\theta-\phi_2, \phi_2), \ldots$, etc. (Relation 1)

Any one of these stimulus patterns gives rise to the perception of a line in position $L(\theta)$. (Relation 2)

It is shown in Note A at the end of the paper that relation 2 can be written in the form

The stimulus pattern (ψ, ϕ) gives rise to the perception of a line in position $L(\psi+\phi)$ (Relation 2′)

The property of giving rise to the same perception defines an equivalence amongststimulus states which we refer to as a Perceptual Equivalence Relation. This relations tructures the set of stimulus states into *equivalence classes*. We will use the symbol $E_1(\theta)$ to denote the equivalence class

$$(\theta-\phi_1, \phi_1) \equiv (\theta-\phi_2, \phi_2) \equiv \ldots.$$

We note that two stimulus states (ψ_1, ϕ_1) and (ψ_2, ϕ_2) belong to the same equivalence class $E_1(\theta)$ if, and only if, $\psi_1+\phi_1=\psi_2+\phi_2(=\theta)$.

III. ROLE OF EXPERIENCE IN PERCEPTION

The terminology we have developed allows us to pose in a very precise manner one of the main questions that divide nativists from empiricists. Can the subdivision of the set of stimulus states into equivalence classes be accounted for in terms of the anatomical structure of the nervous system, or does it depend on the experience of the organism? We are able to answer this question categorically by reference to experiments in which perceptual equivalence relations are changed in a predictable way. The existence of a process by which the existing equivalence classes can be disrupted and replaced by a different set provides a strong presumption that the original ones came into being by the same process and are not innate.

The constancy discussed in the previous section can be altered by the protracted wearing of a pair of spectacles which, by means of an arrangement of mirrors or total reflecting prisms, reflects the visual world in one plane. For example, prisms with their bases in the horizontal plane of the head will cause the world to be seen upside down but will not disturb left-right relations, while prisms with their bases parallel to the medial plane will reverse left-right but not up-down. Unlike the system of lenses used by Stratton (which reverse in both these directions simultaneously) an arrangement of this type produces an instability of the perceptual field when the head is tilted. Fig. 3 shows the effect of reflecting the perceptual world by means of prisms with bases in the horizontal plane. With the head erect (Fig. 3*a*) the world is seen upside down. When the head is tilted through the angle ϕ (Fig. 3*b*) the image of $L(\theta)$ makes an angle $\phi-\theta$ with OX, thus giving rise to the stimulus state $(\phi-\theta,\ \phi)$ which belongs to the equivalence class $E_1(\phi-\theta+\phi)=E_1(-\theta+2\phi)$. Thus a rotation of the head through ϕ causes an apparent rotation of the external world through 2ϕ. The description of the *direction* in which this apparent movement takes place needs some care since the following two statements are not always equivalent:

(P) The world appears to rotate in the same direction as the head but through twice the angle.

(Q) When the head moves through an angle ϕ the subject sees the same movement as he would were his head kept stationary and the line $L(\theta)$ rotated through an angle -2ϕ to the position $L(\theta-2\phi)$.

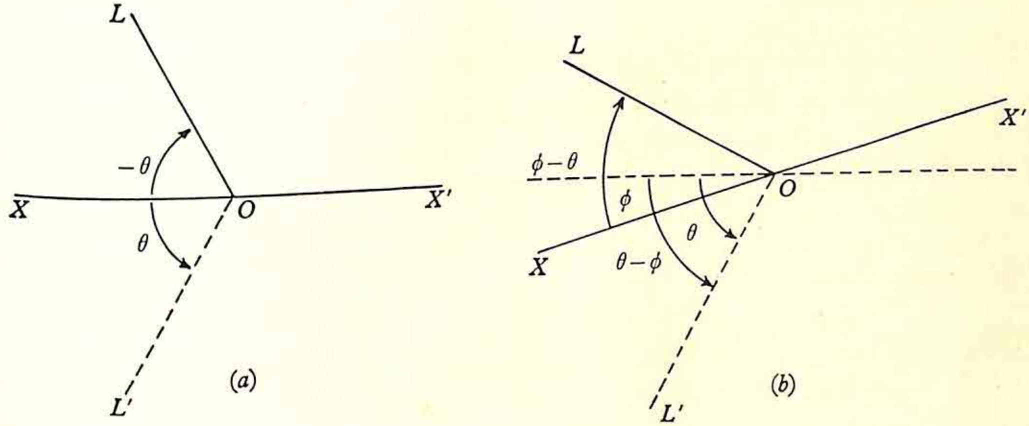

Fig. 3. The retinal field with inverting spectacles, when the head is (*a*) erect, (*b*) rotated through the angle ϕ. The broken line OL' shows where the image of $L(\theta)$ would lie without spectacles.

The need to make the distinction between these two modes of description will become apparent when we discuss the effects of protracted wearing of spectacles.

In experiments conducted by Erismann and Kohler and by the present authors (Kohler, 1951, 1951*a*; Rakoff, 1953) spectacles of this kind* have been worn for periods ranging from a few days to 10 weeks. All subjects who have worn the spectacles for sufficiently long periods report that eventually the instability induced by the spectacles disappears—rotation of the head no longer induces any apparent movement. *Nearly* all subjects also eventually report that the perceived world no longer appears to be inverted. Our main concern in the following discussion will be with the first of these phenomena, though we shall have to refer to the second in assessing experimental results.

A subject who perceives the world as stable while wearing the spectacles has established a new set of equivalence classes (i.e. new constancy relations) which may be represented by

$$E_2(\theta)=\{(\phi_1-\theta,\ \phi_1)\equiv(\phi_2-\theta,\ \phi_2)\ldots\}.$$

This time the relation of invariance takes the form

$$(\psi_1,\ \phi_1)\equiv(\psi_2,\ \phi_2)\quad \text{if, and only if}\quad (\phi_1-\psi_1)=(\phi_2-\psi_2).$$

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* What we have to say applies *mutatis mutandis* to left-right reversing as well as to up-down reversing spectacles. Hence we shall not distinguish between these two types in the following discussion.

This last condition is satisfied by any subject who sees the world as stable, *irrespective of whether he describes the world as inverted or not*. In both cases $(\phi_1-\theta, \phi_1)$ and $(\phi_2-\theta, \phi_2)$ will give rise to the same perception—the difference is that in the one case this will be a perception of a line in the position $L(-\theta)$ and in the other case a line in position $L(\theta)$.

We now turn to the deduction of what must happen when the spectacles are removed after adaptation. Reference to Table 1 shows that if the equivalence classes E_2 are operant, then the subject must see the world tilt through an angle 2ϕ when he tilts his head through an angle ϕ. Moreover, he will see the line tilt in the same direction as that which would be produced by tilting the line in the opposite direction with the head kept fixed. Thus if we use the form of description referred to above as (Q), the *after-effect instability is exactly the same as that observed on first putting on the spectacles*. If, in addition, the subject has adapted in such a way that he not only sees the world as *stable* through the spectacles but also as *non-inverted*, then he will see the world as inverted when he removes the spectacles, and he will describe the apparent movement produced by tilting his head as being in the same direction as the movement of his head. Thus the form of description (P) would also apply. However, if he sees the world as stable with the spectacles, but *continues to see it as inverted*, description (Q) but not description (P) would apply to the after-effect.

Table 1. *States of stimulation (rows* 1 *and* 4) *as determined by* (*a*) *direction of the object line,* (*b*) *position of head, and* (*c*) *whether spectacles are worn or not; and the resulting perception (rows* 2, 3, 5 *and* 6) *as determined by* (*d*) *the equivalence relation that is in operation at the time*

Equivalence relation in operation	Entries in following columns describe	Line at $L(\theta)$, head erect	Line at $L(\theta)$, head at ϕ	Line at $L(\theta-2\phi)$, head erect
		Without spectacles		
—	State of stimulation	$(\theta, 0)$	$(\theta-\phi, \phi)$	$(\theta-2\phi, 0)$
$E_1(\psi, \phi)$	Perception	$\theta+0=\theta$	$\theta-\phi+\phi=\theta$	$\theta-2\phi+0=\theta-2\phi$
$E_2(\psi, \phi)$	Perception	$0-\theta=-\theta$	$\phi-(\theta-\phi)=-\theta+2\phi$	$0-(\theta-2\phi)=-\theta+2\phi$
		With spectacles		
—	State of stimulation	$(-\theta, 0)$	$(\phi-\theta, \phi)$	$(2\phi-\theta, 0)$
$E_1(\psi, \phi)$	Perception	$-\theta+0=-\theta$	$\phi-\theta+\phi=-\theta+2\phi$	$2\phi-\theta+0=-\theta+2\phi$
$E_2(\psi, \phi)$	Perception	$0-(-\theta)=\theta$	$\phi-(\phi-\theta)=\theta$	$0-(2\phi-\theta)=\theta-2\phi$

Note that the third row, which represents the after-effects of adaptation to the spectacles, is the same as the fifth row, representing the first effects of wearing the spectacles.

The experimental verification of these deductions is incomplete. At the time they were made* they were apparently in flat contradiction to the only evidence available. Kohler (1951*a*) had reported that after removing the spectacles the subject saw a tilt which was in the opposite direction to the movement of the head. No test in the form (Q) was applied. In our own experiments we had sacrificed the opportunity of making this observation by adapting our experimental design to answer certain other questions, having, at that time, no reason to be dissatisfied with Kohler's result. However, a re-examination of Kohler's observation shows two features which indicate that it might be quite irrelevant in the present context. First, he saw tilting only when rapid movements of the head were made and these movements were not functionally related to the extent of the head movement—

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* A preliminary draft of this paper was circulated in duplicated form in January 1954.

they were more similar to the flick instability which can be observed by the reader if he makes a rapid jerking movement of his head. Secondly, the observation was not immediately made after removal of the spectacles. For these reasons we consider that they cannot be regarded as simple after-effects of the type under discussion.

A later experiment conducted at Innsbruck in the spring of 1954 yielded results which were in accordance with our prediction, though they do not completely verify it. In this experiment the subject wore his spectacles for 71 days. He reported* stability under head rotation but continued to report reversed vision to the end of the experiment. On removing the spectacles he reported that the world tilted when he tilted his head, that the tilt was functionally connected to the extent of head tilting and that it was in the direction opposite to that of the head movement. This is what our deductions predict for a subject whose vision with the spectacles remains reversed. No test in the (Q) form was applied.

Further experiments on this point have been planned. It should be noted that apart from its importance in testing the validity of the present approach (which must collapse unless this prediction is true) the existence of an after-effect which is identical with the original effect of putting on the spectacles is itself interesting. For this reason we have adduced an *ad hoc* argument which reaches the same conclusion in a different and less satisfactory way. This will be found in Note B at the end of the paper.

IV. SOME OTHER SPECTACLE EXPERIMENTS

The process of altering equivalence classes and so constancy relations is further illustrated by the following experiments, the details of which will be reported elsewhere.

(1) In an experiment conducted by Taylor, Papert and Schermann in July–August 1953 the subject (Papert) wore left-right reversing spectacles for 8 hr. a day. In this case the relevant stimulus states have to be specified by three variables, viz. the retinal stimulation, the proprioceptive stimulation, and a third variable which takes on the values 0 and 1 corresponding to 'spectacles off' and 'spectacles on' respectively. It was found that stability could be achieved both with and without the spectacles. In the restricted situation described in §II the stimulus states may be represented by the triplet (ψ, ϕ, x), where x is the parameter corresponding to spectacles on or off. The equivalence relation achieved during the experiment can be written

$$(\psi_1, \phi_1, x_1) \equiv (\psi_2, \phi_2, x_2)$$

if, and only if, $$(\phi_1 + (1-x_1)\,\psi_1 - x_1\psi_1) = (\phi_2 + (1-x_2)\,\psi_2 - x_2\psi_2).$$

Incidentally, it was also found that 'correct' perception was reported both with and without the spectacles.

(2) An experiment conducted by Papert in Cambridge during January to February 1955 provides a number of examples of the alteration of constancies by the use of spectacles consisting of 45° prisms arranged as in Fig. 4. The effects of such prisms are rather complex, but two fairly simple examples can be discussed without going into detail.

(a) A suitable tilting movement of the head will cause the field to move upwards when the head goes to the left and downwards when the head moves to the right. The extent of this movement is not uniform in different parts of the field, and so a complicated distortion is induced. However, this aspect will be ignored here. After 5 weeks of life in the spectacles, this apparent movement had considerably decreased, though it still remained in certain circumstances. When the spectacles were removed an after-effect movement was observed. This was in the direction opposite to that originally induced by the spectacles, but considerably less in extent and not always very clear. However, when the test was carried out in a dark room with only a luminous point visible, the extent of the after-effect movement was approximately equal to that of the original effect of the spectacles, though in the opposite direction. We suggest that when the full field is viewed with full illumination there are numerous components of the situation which are able to act like the parameter x in the previous example, so that the 'spectacle' habits have to compete with 'pre-spectacle' habits evoked by these cues. In the dark no such situation obtains and the most recently established set of habits operates unhampered. (This observation may be relevant to the problem of after-effects with reversing spectacles referred to in the previous section.)

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* Personal communication by I. Kohler.

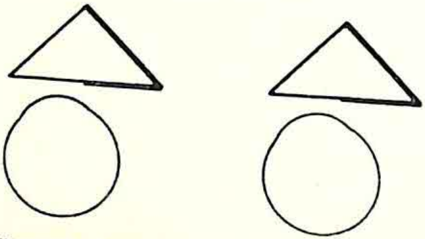

Fig. 4. 45° prism spectacles. Heavy lines indicate blacked-out surfaces.

The fact that the after-effect movement should in this case be in the direction opposite to the original movement is easily deduced by drawing up a table similar to Table 1. (See also Note C.)

(*b*) When the spectacles are first worn there is a very marked compression effect—as objects move to the right of the visual field they are increasingly shortened in their horizontal dimensions. Thus a square in a suitable position will appear to be a rectangle with its vertical side longer than its horizontal side. If it is rotated it appears to change its shape, the apparently longer side becoming apparently shorter and vice versa. After adaptation to the spectacles a reverse effect is seen when they are removed—a horizontal line of length 5 in. will appear to be equal in length to a suitably placed vertical line of length 6 in. Here we have a new constancy relation set up which differs from those previously discussed in that it does not depend essentially on a proprioceptive factor.

The equivalence relation for length of a line segment must be written in the form

$$(x_1, f(\chi_1)) \equiv (x_2, f(\chi_2)),$$

where x_1 is the length of the retinal image of the segment, χ_1 is the angle it makes with the retinal axis and f is a suitable function.

This case illustrates the fact that the kind of analysis used above applies also when the variables are not simply 'retinal stimulation' and 'proprioceptive stimulation'. However, the mathematics involved is somewhat more complex and will be published fully elsewhere.

V. LEARNING TO SEE

We now have to pose the question: by what process do the equivalence classes come into being? In our discussion of the equivalence classes underlying the constancy of the visual field when the head is tilted we identified a mathematical invariant—$\psi+\phi$ in the case of a normal subject, $\phi-\psi$ in the case of a subject adapted to reversing spectacles. Our problem is to discover a physical invariant corresponding to this mathematical construct which is responsible for bringing together the various stimulation states which will eventually form an equivalence class. The nativist approach leads one to seek this in the structure of the pattern of stimulation or of the brain process aroused by it. Examples of this approach are the models which have been proposed by the Gestalt 'field theory' psychologists, by McCulloch (1951) and by Deutsch (1955). Each of these theories postulates a mechanism, anatomically determined, which will actually reduce all the members of the same equivalence class to a common canonical form. It seems clear from the spectacle experiments that while such mechanisms may exist, they cannot provide the answer to the problem which they are designed to solve. Our hypothesis is that the physical invariant underlying the equivalence classes is not an intrinsic property of the stimulation states or of the anatomically determined brain processes set up by them, but is to be found in the set of responses with which these stimulus states have been associated through the individual's commerce with the external world.

Consider, for example, our original subject with his reversing spectacles. Suppose that he sits in front of his blackboard and tries to trace with his fingers the various lines $L(\theta)$. If we denote by $R(\theta)$ the action of moving the finger in the direction of the line $L(\theta)$, we find that his initial tendency is to respond to $L(\theta)$ with the movement $R(-\theta)$. This will not be successful and he will try various other responses until eventually he hits on the correct one $R(\theta)$. After some practice he will set up a conditioned response which may be represented by

$$(-\theta, 0) \rightarrow R(\theta).$$

He now tilts his head to the position ϕ_1. The stimulus pattern will change from $(-\theta, 0)$ to $(\phi_1-\theta, \phi_1)$, and he will again make a series of incorrect responses until he finds and practises the correct one which is again $R(\theta)$. Eventually the patterns of behaviour

$$(\phi-\theta, \phi) \rightarrow R(\theta)$$

will be established for all values of ϕ, and so the set of stimulus states of the form $(\phi-\theta, \phi)$ will have been associated *through* the common response $R(\theta)$. Moreover, this association would be reinforced by *any* other mode of behaviour; at least on the overwhelming majority of occasions the response appropriate to $(\phi_1-\theta, \phi_1)$ is also appropriate to $(\phi_2-\theta, \phi_2)$ and vice versa. Our hypothesis is that the constancy is due to the accumulation of associations of this type and its extent a function of the extent to which the association is invariable.

The last remark may be illustrated by reference to the vexed question of the constancy of a rectangle (say a matchbox) which moves in such a way as to make the retinal image increasingly unrectangular. Whatever the angle of view of the matchbox, the attempt to lift it requires that the fingers be held the same distance apart, holding it in the palm requires the same degree of closure of the hand, etc. However, the constancy of shape is not as absolute in this case as in the case of vertical lines which remain vertical when the head is tilted. One reason for this may be that while there is a common element to the responses made to the matchbox in various positions, there is also a difference which is more appreciable than the difference which exists in the other case.

Note A. Suppose the line which gives rise to the stimulus state (ψ, ϕ) is $L(\theta)$. We know from relation 1 that $L(\theta)$ gives rise to stimulus states of the form $(\theta-\phi, \phi)$. Thus $(\psi, \phi)=(\theta-\phi, \phi)$ i.e. $\psi=\theta-\phi$ or $\theta=\psi+\phi$.

Note B. Suppose (if possible) that X is a subject who has *completely* adapted to the spectacles (say by wearing them for 50 years). Then the effect induced by putting another pair of similar spectacles on X *over* his experimental pair will be the same as that induced by putting a pair of spectacles on to a fresh subject Y. Thus $X+$two spectacles$=$ $Y+$spectacles. But simple optical considerations show that two prisms in tandem are essentially the same as no prisms, i.e. this system will produce neither inversion nor instability of the type under discussion. In other words $X+$no spectacles$=Y+$ spectacles. Q.E.D.

Note C. The argument of Note B does not apply to this case, since two prisms of this type arranged in tandem will *not* be equivalent to the situation of no spectacles. To annul the effect of one of these prisms it is necessary to have a prism of opposite orientation, and the perceptual effect of such a system is to produce instability movements in the opposite direction.

It should be noted that while arguments of this form are often useful they must be used with caution lest the blanket assumption of *complete* adaptation beg the question.

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PUBLICATIONS RECENTLY RECEIVED

Human Personality and its Survival of Bodily Death. By FREDERIC W. H. MYERS. New York and London. 1954. 2 vols. 105*s.*

This monumental work, first published in 1903, has had great influence on the development of psychical research. It has not been much read by psychologists. One reason for this is made clear in the opening of Myers's Introduction, in which he speaks of his object as that of inquiry into whether there is any element of personality which survives death. Although this is certainly a momentous question, it is one in which experimental psychology has not yet interested itself, and, to many experimental psychologists, it seems certain that it never will. It is, nevertheless, a question in which William James was interested, and the sympathetic introduction to the present reprint by Gardner Murphy is evidence that some modern experimental psychologists share this interest.

While this was Myers's main intellectual problem, he ranged widely over the field of psychical research. He was one of the initiators of the experimental study of telepathy, although experimental work in this field has carried the subject much further than it was in Myers's time. Although his training was in the Classics and not in the Sciences, Myers had read widely in the psychology of his time, particularly in psychopathology. He is said to have been the first to introduce the early work of Breuer and Freud to the English-speaking world and was interested in the work of Charcot when few other people were. His extensive glossary of psychological and psychopathological terms gives evidence of the width of his reading in these subjects.

But if he was successful in learning about the science of psychology as it was in his time, Myers was perhaps less successful in mastering the ways of thought characteristic of science. This does not mean that he was not thorough in his collection of data and rigid in his criteria of evidence. His voluminous case histories and appendices have been a mine from which other students have dug out facts ever since, and no one could have been more careful than Myers not to draw conclusions beyond those justified by his data. What seems unfamiliar to the scientific reader is the lack of method in Myers's conceptualization and his tendency to use emotive language. For example, in putting forward the very prosaic idea that the essential element of poetic excitation lies in scarcely perceptible changes of tension in the muscles of the throat, he goes on to say that they so 'flood the flats of common consciousness as with the earthquake-wave of an unfathomed sea' (vol. I, p. 102). For better or worse, that is how scientists have been trained not to think, and references to 'unfathomed seas' and 'subliminal uprushes' are inclined to create a prejudice in the mind of the scientific reader which may not be altogether reasonable.

Myers was not a scientific psychologist but a classicist who knew a great deal about scientific psychology. We must bear with the fact that he thought like a classicist and appreciate that he applied his thinking to a number of very interesting problems which have been relatively neglected by those with a more rigorously scientific cast of mind. His book deserves wide reading. There may be some superfluous ornaments to the thought, but the matter is important and well documented. We may decide that nothing can survive bodily death, but the grounds for doing so must be somewhat uncertain until we have read Myers's case for regarding survival as a possibility.

R. H. THOULESS

Über Geschehensgestaltungen in der Auffassung von Filmen durch Kinder. By I. GERHARTZ-FRANCK. Leipzig: Johann Ambrosius Barth-Verlag. 1955. Pp. iv + 124. DM. 7.20.

This slim volume constitutes the third in a series edited by Prof. Gottschaldt (Berlin) on child development. The present research investigates the way in which children of different age and intelligence levels perceive and comprehend films. The orientation underlying the study is that of Gestalt psychology. The study is an excellent one; it combines the ingenuity and freshness of Piaget's approach with the rigour of experimental design. The author showed a short film of the Beatrix Potter story of the town mouse and the country mouse to groups of children ranging from 6 to 15 years of age. Immediately after seeing the film, the children were asked to describe it. Interesting age differences occurred; the 6-year-olds, for example, disregarded the structure of the film, the psychological problems portrayed and simply enumerated a number of scenes, giving prominence to scenes describing activities with which they were familiar, such as washing, eating, etc. With increasing age, more details were remembered and a more chronologically correct picture presented. It was only at about 12 years of age that major attempts at interpretation were made. It is of particular interest that, after the age of 11, the wealth of detail reported decreased with increasing age—comprehension of the structure of the film led to a decrease in the reporting of minor incidents which are not closely linked to the central theme.

An experimental variation of the film, made by interspersing an irrelevant film sequence, led to greater clarification of the way different age groups structured their perception of the film. Other techniques used were those of delayed recall and of recall aided by presenting the child with stills of the major scenes. A second presentation of the film led to descriptions of the film by the 6-year-olds in a manner characteristic of the 8-year-olds, and similarly the presentation of the 8-year-olds appeared more mature. The author points out implications for the use of films as a teaching medium for young children.

HILDE T. HIMMELWEIT

The Journal of Analytical Psychology. London: Tavistock Publications, Ltd. 1955. Vol. I, no. 1. Pp. 108. 21*s*. per volume of two issues.

It is an excellent plan that a new journal of psychology should have been started under the independent sponsorship of the Society of Analytical Psychology, with the collaboration of C. G. Jung, and it is to be hoped that the example will be followed elsewhere in Britain. This issue includes articles by J. Layard, R. Hobson, R. Moody, L. Stein and A. Allenby, with an editorial note, a short communication by A. Storr and reviews of Jung's works.

Although the journal will not please the statistical and experimental workers on clinical psychology, personality and social psychology, it will appeal strongly to those for whom Jungian theories and insights are important, and it is satisfactory that their ideas should have an independent channel of communication in Britain.

R. W. PICKFORD

Contributi del Laboratorio di Psicologia, Series XVIII and XIX. (*Pubblicazioni dell' Università Cattolica del S. Cuore*, Vols. XLVIII and XLIX.) Milan: Vita e Pensiero. No date. L. 7000 each.

These volumes contain reports of some of the work carried out between 1952 and 1954 in the highly reputed Laboratory of Experimental Psychology at the Catholic University of the Sacred Heart, Milan, directed by Fr. Agostino Gemelli. Although most of the papers are in Italian, some are in English, French or German.

A paper by Gemelli on 'The perplexities of modern psychology' expounds the view of psychology which is presumably prevalent in his Department. He has unfavourable things to say about idealist philosophers, including such revered figures as Croce and Gentile, who shun scientific psychology as an upstart of positivist or materialist parentage and offer their own bad psychology as a substitute. At the same time, he takes to task Wundtian atomism, psychoanalysis and the testing movement, while accepting their partial validity, on the grounds that they do not study the 'whole man'. Those psychologists who trespass on philosophical territory, usually by offering materialist solutions for the mind-body problem, are harshly dealt with, and his strictures against the Cartesian tradition are severe. He insists on the psychophysical unity of the human organism and draws sustenance for this position from the writings of Aristotle, Aquinas, Rosmini and the Gestalt psychologists, although his vitalist leanings would surely not have the concurrence of the last-named.

The other contributions testify to Gemelli's own versatility, to the range of interests represented among his colleagues and to their eagerness to combine high scientific standards with Catholic orthodoxy. The principal fields are industrial psychology, personality tests, sensory thresholds, perception, reaction-times and the spectrographic analysis of speech. Zavalloni produces an interesting synthesis of the Rogerian technique of psychotherapy and the Catholic conception of the act of choice, and adds to current attempts to measure changes under therapy objectively. A great deal of acquaintance with recent American and German work is shown, and some of the papers are about adaptations of well-known American tests. Isolated ideas and devices from the U.S.A. have been taken over, but there are few signs of an interest in the wider methodological and theoretical issues with which these are bound up. On the whole, the point of view is rather like one that was until recent years quite prevalent in Great Britain and still has its adherents—a conception of psychology as a collection of disparate problems and a disinclination to search for any but the most elementary unifying principles, a highly accomplished preoccupation with experimental techniques combined with an indifference to sophisticated theoretical techniques.

The price of these books—about £3. 10*s*. each at the current rate of exchange—is unlikely to turn them into best-sellers among English-speaking psychologists. But they form a worthwhile acquisition for libraries and a means of keeping in touch with the work of one of the most important continental centres of experimental psychology.

D. E. BERLYNE

Conceptions of Modern Psychiatry. By HARRY STACK SULLIVAN. London: Tavistock Publications. 1955. Pp. xiii + 298. 32*s*. 6*d*.

Tavistock Publications are to be commended for making available to British readers this work, hitherto unpublished in this country. It contains the five lectures that Sullivan delivered in 1939 as the William Alanson White Memorial Lectures, with a further chapter on Sullivan's theories by Patrick Mullahy. In these lectures are to be found a restatement of Sullivan's standpoint and views, although he himself felt that they were an inadequate account of his position.

For Sullivan, psychiatry is the study of interpersonal relationships, and the ultimate causes of mental disorder, he believed, are to be sought always in the social order itself. Starting with his basic conceptions, he goes on in this work to give a brief but, nevertheless, comprehensive account of the emotional development of the child. Here we encounter Sullivan's ideas on empathy, his concept of the self dynamism and his concept of anxiety. He uses some unfamiliar terms to expound his ideas, but his basic formulation, the relating back of all emotional and mental illness to the field of interpersonal relations, is never obscured. His theories, moreover, are rooted in biology, and he stresses the continuity between the biological and the cultural.

He is perhaps at his most fascinating in discussing schizophrenia, a condition which he divorces entirely from the organic condition dementia praecox. He discusses schizophrenia from a truly dynamic standpoint as a 'disorder of living', and contrasts the standard Kraepelinian formulation with his own. If only for this essay alone, this book is worth reading. Even the most conservative cannot fail to be impressed by the author's grasp of the patient's mode of thinking and the way he seems to understand the schizophrenic's inner situation. As might be expected, physical methods of treatment are frowned on by the author—they reduce the patient's capacity for being human, he writes—and he makes his case as convincingly as anyone in this field. This work will repay study and stands as a fitting memorial to a great American psychiatrist.

H. I. CLAPHAM

The Psychiatric Interview. By HARRY STACK SULLIVAN. London: Tavistock Publications. 1955. Pp. ix + 246. 28*s*.

This is the second posthumousp ublication of Sullivan's work by the William Alanson White Psychiatric Foundation. The book is based on recordings and notes of a lecture series given by Sullivan, and to its advantage much of the lecturer's pithy, racy style is retained.

The title might suggest that this is for specialist consumption only. However, psychiatry to Sullivan is the field of study of interpersonal relations and its core is the interview situation, a special instance of interpersonal relations. Thus Sullivan's work is directed not only to the specialist in mental or nervous disorders but to all those who can be considered experts in interpersonal relations. A subtitle specifies social worker, personnel manager and counsellor. The reviewer would have liked to see the list include psychologist, even though the criterion of expertness in human relations might prove embarrassing. Psychology tends to make us only too aware of the technical faults of interviewing, but it is unfortunate that we should just accept the deficiencies of the procedure and seek other more valid techniques, rather than investigate ways in which the interview could be improved as a device.

Sullivan's work is really a highly personal account of his particular interviewing skill, but his analysis and description of the interpersonal processes between interviewer and interviewee stress the importance of a 'two-person psychology' of interviews. This idea alone could well provide the basis for a re-examination of the scientific status of interviewing. Sullivan's minute analysis of interpersonal processes seems to be scientifically fruitful as well as an aid to professional interviewing.

Thus the book could be read to advantage by psychologists other than clinical psychologists. Clinical publications are already appearing in which Sullivanian ideas of interpersonal relationships are being examined experimentally. This book is recommended as a readable introduction to Sullivan's philosophy as well as an interesting analysis of a professional skill which is too often disparaged or concealed as 'intuition'.

R. M. MOWBRAY

The Interpersonal Theory of Psychiatry. By HARRY STACK SULLIVAN. London: Tavistock Publications. 1955. Pp. xviii + 393. 35*s.*

This book is one of the trilogy of Sullivan's works recently published in Britain. It is a compilation of his lectures and unpublished writings, amplified at certain necessary points by editorial comments referring to the author's earlier work. The result is a highly significant exposition of the modern psychodynamic approach of the Washington–Baltimore school of psychiatry.

A wide variety of topics is dealt with in this volume, which is divided into four parts. Part I deals with basic concepts and, in particular, with Sullivan's developmental approach to psychiatry. In his view the most worth-while understanding of sick and healthy human beings is derived from a detailed consideration as to how 'from birth onward a very gifted animal becomes a person'. Sullivan maintains that grasp of the significance of anxiety is fundamental to the psychiatrist's undertaking of valid therapeutic work with patients. While he does not embrace the semantics of Freudian ego-psychology, his system demands the learning of a private language. He pays less attention to the minutiae of psycho-sexual development than to the concept of interpersonal interaction taking place in a social field.

Part II represents the bulk of the book, and here the author considers the emotional significance of the various developmental epochs from early infancy to late adolescence in a systematic and detailed fashion. The orientation is a broad one, attempts being made where possible to correlate psychological findings with neuro-physiological data; for instance, when discussing the infant's psychological differentiation of his own body, Sullivan stresses the importance of the manual-oral co-ordination in the development of body libido. This emphasis upon 'two-zone co-ordination' is one of the keystones of his developmental theory. Finally, in this section, the author states his views on emotional maturity and on a sense of self-respect as a necessary condition of maturity.

In Part III the most arresting contribution concerns Sullivan's attitude to schizophrenia. Here he regards schizophrenia as a defensive position or way of life adopted by the patient who is unable to deal otherwise with traumatic situations ('not-me experiences') that arouse intolerable anxiety. The idea that schizophrenia represents 'a way of life' does not really amount to a theory of schizophrenia; it is rather a viewpoint which chooses to disregard such possible factors as genetic predisposition or immaturity of neural organization.

Part IV is devoted to a consideration of the relevance of social and cultural factors to psychiatry as a whole. Here, Sullivan moves away from psychiatry as a clinical speciality towards a psychiatry which purports, a little grandiosely, to become 'a science of living' and which is more intimately linked with the social sciences. In this connexion, he points out how this 'science of living' imposes a duty upon both psychotherapist and patient to foresee, if possible, the near future as well as the past.

This book is not only of inestimable value to the practising psychoanalyst but also to any psychiatrist who would achieve a dynamic understanding of his patients. Despite occasional obscurities of expression the book provides a highly stimulating account of Sullivan's humanism and of his weighty theoretical contributions to psychiatry.

A. BALFOUR SCLARE

Fragments of an Analysis with Freud. By JOSEPH WORTIS. New York: Simon and Schuster; London: Collet's Holdings Ltd. 1954. Pp. xii + 208. 18*s.*

This book contains a diary of what transpired during the course of a 'didactic analysis' which Dr Wortis underwent with Freud during the four months October 1934 to January 1935 inclusive. He assures us that, whilst he has bowdlerized the account, he has not distorted it; and the chief reason which he gives for the bowdlerization is that he has 'no wish to achieve distinction by posing naked in the street'. On the assumption that this consideration is to be taken at face value, it would be difficult to explain his wishing to publish the diary at all, were it not for the fact that it is obviously intended to be a study of Freud rather than of himself. The analysis was undertaken while Dr Wortis was in Vienna as a research scholar studying sex problems; and evidently he regarded the circumstances as providing a favourable opportunity for a Cook's tour of psychoanalysis. What induced Freud to accept him as an analysand for a prescribed period of four months is only one of the many puzzling aspects of the account; for the surface of the unconscious could barely be scratched in such a short period. The term 'didactic analysis' is also puzzling, since to be didactic is foreign to the aims of a personal analysis. However, if the account provides a fair picture of what occurred during sessions, Freud must have been the least orthodox of all analysts; and the sessions are represented as consisting largely of theoretical discussions in which Freud adopted an unmistakably didactic attitude and engaged in active controversy with his analysand. Indeed, Freud is represented as breaking almost every rule of analytical technique; and, as regards his interpretations of the analysand's dreams and associations (so far as these are recorded), it is difficult to believe that the founder of psycho-analysis could be so obtuse. In general, one cannot help feeling that, however factual the account may be, its publication is in doubtful taste, even if it does not involve a breach of that discretion which the need for mutual confidence imposes on both parties to an analysis.

W. RONALD D. FAIRBAIRN

Eros and Civilization: a Philosophical Inquiry into Freud. By HERBERT MARCUSE. London: Routledge and Kegan Paul. 1956. Pp. xii+277. 25*s.*

Freud was led by the technical utility of his concepts of instinct and repression to suggest that civilization is possible only when the former is deprived of direct satisfaction by the latter. This forced him to conclude that his technique, which he conceived as the reduction of repression, could never be completely successful, even in theory, since the patient must continue to be a member of society and so must continue either to repress the more antisocial of his impulses or endure the tension of conscious self-denial. This pessimistic and somewhat *a priori* reasoning was attacked by Fromm, who was both a closer student of social history and an optimistic socialist. He questioned several of Freud's fundamental assumptions and suggested, for example, that aggression was not innate but implanted by the frustrations of a particular kind of civilization. The technique of the 'cultural' school of psychoanalysts in the United States stems from his ideas.

Prof. Marcuse dismisses them as 'neo-Freudian revisionists', but devotes an epilogue to arguing that while their theories may be therapeutically useful they are trying to dodge the crucial question: 'Must civilization entail the renunciation of instinctual satisfaction?'

He himself might be classified as a palaeo-Freudian Hegelian. He thinks that Freud's assumption of innate and immutable instincts was right, but that his conclusion was unnecessarily pessimistic. He argues, in the first place, that the instincts, and particularly *eros*, the pleasure-seeking group, have a natural tendency to sublimate themselves, and do not merely do so, as Freud thought, under the compulsion of civilization. His second argument is that Freud confused two kinds of repression. Repression proper is necessary if man is to live at all, let alone in society. What is imposed by social institutions is 'surplus repression' (although, as it is described by Prof. Marcuse, this sounds more like conscious or habitual obedience to restrictions).

He goes on to point out that both inside and outside society men have had to forgo instinctual satisfaction out of the sheer necessity of performing unpleasurable work; since he regards this as a special instance of the reality-principle he calls it the 'performance-principle'. But when civilization attains such a high degree of productivity that this 'alienated labour' is reduced to negligible proportions, this state of affairs will combine with the naturally sublimated *eros* to produce the 'freedom of a non-repressive civilization'.

If I have over-simplified a solution, particularly of a crucial problem, my excuse must be that Freud and Hegel make a confused duet, without the accompaniment of any concrete illustrations. It is true that the polarities to which Freud tended (ego and id, eros and thanatos, reality- and pleasure-principles) lend themselves to dialectic, and that Prof. Marcuse has a great deal of insight into both. But Freud's sociological assumption was only the speculative by-product of a therapeutic technique: Fromm was the sociologist, and it is startling to find his theories praised for their therapeutic value but dismissed in their proper field in favour of Freud's.

NIGEL WALKER

Psychotic and Neurotic Illnesses in Twins. By ELIOT SLATER, with the assistance of JAMES SHIELDS: Statistical Appendix by JOAN MAY. Medical Research Council Special Report Series, No. 278. London: Her Majesty's Stationery Office. 1953. Pp. vi+385. 21*s.* $4.75.

Dr Eliot Slater is Director of the Genetics Unit of the Institute of Psychiatry, University of London. His investigation was in the first place concerned with the standing population of 20,640 patients in ten hospitals for adult psychotic patients of the London County Council Mental Health Services. But, with two additional hospital sources of twins, 297 twin pairs were finally available for statistical analysis.

In the case of same-sexed twins, the anthropometric examination included measurement of height, of the hair colour (against a standardized range of human hair samples), eye colour (against a standardized scale of glass eyes), four skull measurements and the taking of finger-prints. The use of finger-prints here marks a definite advance in research technique. Dr Slater has worked out 'a method of reducing the total description of finger-prints to purely quantitative characters'.

The 297 twin pairs fall into four main diagnostic groups: (I) 158 pairs of schizophrenics; (II) 38 pairs in the Affective Disorders Group, 23 of whom were manic depressives or primary endogenous depressives; (III) 49 epileptics and organic psychoses; and (IV) 52 pairs of psychogenic behaviour disorders in children, psychopathic reactions occurring in mental defectives, besides other neurotic and psychopathic states. Sex, ovularity and clinical diagnosis of all twin pairs are tabulated. The four groups are compared for (*a*) size of family, (*b*) abnormality in family, (*c*) personality resemblance between uniovular twins, (*d*) twins separated in childhood and later life.

Two of the conclusions in (*b*) may be noted: '...general constitution and non-specific genetical factors are highly important in determining the manifestation of schizophrenia or of psychopathy or neurosis, but less so the manifestations of an affective or organic illness. The environmental hypothesis...cannot be excluded, but it is not plausibly reconciled with the difference between the schizophrenic and affective groups.' Secondly, there is suggested a relationship 'between bodily constitution and powers of resistance to schizophrenia'.

(*c*) Personality resemblance between uniovular twins—15 male and 39 female pairs—is discussed in some detail. 'In the absence of any pathological change, [they] develop closely similar personalities'.

(*d*) Only three pairs of twins were separated from early childhood until after they had left school. In only two cases, one binovular and one uniovular, 'is there anything like clear evidence that the different experience of the twins...contributed anything significant to their differing psychiatric fates'.

Factors predisposing to or associated with mental illness are tabulated and discussed. Unfortunately, as Dr Slater says, information is often scanty and not all twin pairs can be used. Psychologists of the extreme environmental outlook will need to ponder the finding that 'there is a marked tendency for the more severely affected twin to have been the more neurotic child, but this tendency, however, is shown only by the binovular twins....This suggests that the difference is genetically determined and that the constitutional make-up which predisposes to mental illness in the adult helps to cause neurotic symptoms in childhood'. As for neurotic traits in adult life, 'we can suggest first that the make-up of the personality affects the liability to psychotic as well as neurotic illness, and that environmental as well as genetical factors play a part in its causation'.

In Part I of the Report, Slater discusses in 22 pages twin investigation in psychiatry—its history, rationale and validity. He critically summarizes previous work, much of it in German, on unselected series of psychiatric twins.

Part III is the core of this investigation: the clinical investigation of the data on the four psychiatric groups. For each group in turn, duration, type of onset, precipitation, clinical symptoms and treatment are discussed.

Abnormal personalities found among first-degree relatives of the propositi in the four main groups were classified. Abnormal personalities in the schizophrenic families (75%) were much more markedly abnormal than those found in the affective families (54%). This may be an artifact. A colleague of the reviewer has recently found that different types of psychiatric patients differ in the extent to which they report psychiatric illness in the family history.

Slater writes in the epitome: '...genetical causes provide a potentiality for schizophrenia, perhaps an essential one, though environmental factors play a substantial role, which may be decisive in the individual case. Those environmental factors which affect personality and constitution appear to be the most important. Heredity is also an important factor in the causation of affective and organic psychoses.' 'There is a striking contrast between the psychoses on the one hand and the psychopathic and neurotic states on the other. In the latter, uniovular pairs were less frequently concordant, and some of the binovular pairs developed very similar troubles despite big differences in intelligence and personality.'

This research deserves the widest circulation. No one should graduate in psychology without some familiarity with its findings. It will help to balance the excessive emphasis on purely psychogenic causation current in many text-books and suggest a more reasoned expectation of 'cure' in psychiatric illness.

The critic may question the categories, the diagnoses and their reliability, and eventually the whole framework of clinical psychiatry as providing concepts suitable for translation into genes. (See Gardner Murphy, 'A research programme for qualitative Eugenics', *Eugen. Quart.* **1**, 209–12, 1954.) In the tradition of the best clinical psychiatry Dr Slater presents full case histories of all pairs of uniovular twins, of all pairs of doubtful ovularity and of concordant binovular pairs, and summaries of all other cases. He is careful to point out where data are insufficient and to give due weight to alternative interpretations. Combined with this is a sensible use of statistics. Wherever possible data are tested for significance.

The psychologist will regret that more quantitative data derived from psychological tests were not available, but it is obvious that this would have involved another full-time research worker. In some case histories I.Q. (in places given as 'I.Q. 116 per cent') is listed, but other statements about intellectual level are presumably clinical judgements.

Dr Slater's research is assured of a permanent place in the world literature on psychiatric genetics.

J. C. KENNA

Training the Backward Child. By HERTA LOEWY. London: Staples. 1955. Pp. 166. 12*s*. 6*d*.

In this book, written for parents and teachers of backward children, Miss Loewy draws upon her first-hand experience of living and working with backward children over a considerable number of years. This follows her previous book, *The Retarded Child*, carrying her ideas further and describing in some detail methods of teaching by means of games, stories, jingles, music and mime. Her claim that she can help the child classed as 'ineducable' is substantiated by the interesting case studies that she includes.

The style is simple and homely, which is rather irritating to the professional reader, but may commend itself to the layman; the sincerity and enthusiasm of the writer are evident.

Miss Loewy stresses the importance of working in close co-operation with the physician and of early physical treatment for the backward child. She writes realistically and sympathetically of the attitudes of parents to their backward children. 'The effect on parenthood of having a backward child is exactly what you choose to make it—make it something constructive that will help the child' is her plea. She has some frank but wise things to say about grandparents.

The chapter on religious education is too brief, and one looks in vain for a clear statement concerning the spiritual needs of the children under her care. However, the importance of simplicity and clarity in presenting religious concepts to the backward child so that 'his use of religion' does not become 'a hollow mockery' is rightly stressed. She includes a number of rhymes, playlets and stories which are devised for the enjoyment of the children, to increase their self-confidence, to improve their social adjustment and their general behaviour. It is questionable how greatly these may influence their moral development, which is one of her aims.

Diets found useful for combating acidity, constipation, undernourishment, obesity and digestive troubles are described at some length. The contention is made that 'a digestively happy child is much more likely to make mental progress'. Considerable emphasis also is laid on the value of exercise.

There are many practical points raised in this book, and much useful guidance given to both parents and teachers. The material is rather fragmentary and not arranged in a very satisfactory way, but the careful reader, who is concerned with both the social and the educational problems of the backward child, will find much that is interesting.

A. H. BOWLEY

Emotional Problems of Early Childhood. Edited by GERALD CAPLAN. London: Tavistock Publications. 1955. Pp. xiv+544. 42*s*.

This book consists of twenty-four papers selected from those presented to the Institute of Child Psychiatry in Toronto in August 1954. It is divided into three main sections: preventive aspects of child psychiatry; the relation of physical and emotional factors and problems of hospitalization; psychosis in early childhood. Each section contains case histories, research reports and a general commentary by such eminent writers as Drs Gerald Caplan, Frederick Allen and Marian Putnam. These commentaries are probably the most valuable part of the work.

The main emphasis of the book is, rightly, on prevention, and on the need to detect and modify conditions in the family and in the wider social environment that are likely to lead to pathology in the child. Several papers deal with the important problem of devising objective methods of observing and classifying behaviour (including verbal behaviour). It is too early, yet, to judge the value of this research. Stone describes his findings as 'a few tentative impressions and hypotheses and a plethora of questions'. One feels that publication in these cases is perhaps premature. A number of faults appear in the research reports, which even the great difficulty of the task does not justify. These include ambiguous statement of results, wide generalizations drawn from inadequate numbers, and, in one case, a control group of twenty children, ranging in age from 3 to 15 years. In marked contrast to these are the excellent reports of Beata Rank on children showing 'atypical' development, treated at the Putnam Center in Boston during the past ten years, and of Lauretta Bender on twenty years of work with schizophrenic children in Bellevue Hospital, New York.

The case studies, though interesting, present no new developments in either diagnosis or treatment. In all cases, the outcome and, in some, the etiology is in doubt. It is unfortunate that the one chosen to illustrate Mahler's interesting concept of symbiosis in childhood schizophrenia should be that of a child where neither the psychosis nor the symbiotic relationship are satisfactorily demonstrated.

The book is a valuable record of the progress in Child Psychiatry up to 1954, but it is not, as claimed on the dust cover, 'an authoritative statement on the diagnosis, treatment and prevention of emotional disturbance'.

JOYCE COOKSON

La Perception. Symposium de l'Association de Psychologie scientifique de Langue française. Paris: Presses Universitaires de France. 1955. Pp. 128. 500 fr.

The *Association de Psychologie scientifique* devoted its second annual meeting held in Louvain in 1953 to perception. This symposium contains papers read at the meeting and the discussion which followed some of them. The book falls into two parts. Part I has for its theme 'the influence of experience on the organization of sensory data in perception'. It includes papers by Piéron, Piaget and Michotte, and the subsequent exchange of views. Part II is a more heterogeneous collection of four papers: 'Perception and information' by Ombredane, 'Perception and personality' by Nuttin, 'First responses in the infant and their stimuli' by Fauville, and a discussion by Koch of 'a possible link between physiology and behaviour in the migratory fish and birds'.

The theme of the three *rapports* which form the body of the first part of the book is, of course, a lively controversial issue at present. A publication which contains concisely stated views of Piéron, Piaget and Michotte on the subject cannot fail to be of great interest. Piéron breaks his lance in defence of a far-reaching empiricism. His paper offers a review of the evidence supporting this position: experiments on sensory deprivation and on perceptual constancies, and the work of Ivo Kohler. Theoretically, he achieves a not unfamiliar blend of Hebb's ideas and of the Princeton transactionalists' treatment of the role of action in the development of perceptual organization.

Michotte represents the 'nativist' extreme of the range. His paper is a plea for a careful 'structural analysis' of the external determinants of perception. This, as exemplified by Michotte's earlier work on the perception of mechanical causality and his more recent studies on 'substantial permanency', would yield possibilities of predicting the phenomenal percept. A parsimonious explanation of some fundamental aspects of perceptual organization could thus be achieved without recourse to past experience.

Piaget's paper provides a link between Piéron's and Michotte's contributions. He applies to problems of perception his concept of equilibrium which has, according to him, a higher degree of generality than either 'innateness' or 'experience'. One may regret that a more specific account of the operation of laws of equilibrium in perception is not given. In the course of the discussion Piaget mentions that someone has asked him 'what he meant by equilibrium'. His reply is 'reversibility in general'. However, in the context of this paper the notion of equilibrium stands a little isolated, and sometimes may even appear to have the status of a final cause. A number of fascinating experiments are described in the paper, some of them very recent, drawn from Piaget's work on perceptual development in children. They fully support a distinction drawn by Piaget between perceptual potentialities of an organism, which may be innately determined, and the contents of perceptual experience which are based in very large measure on slowly acquired and progressively more complex co-ordinations.

This section of the book is worth reading more than once. In addition to providing a concise and thoughtful exposition of some of the most interesting aspects of contemporary psychology, it also reveals some of the cracks in the foundations. There seems to be a curious lack of communication between the various contributors. Piéron and Michotte carefully select the evidence which they use to support their views; each of them seems to avoid, almost as carefully, a thorough examination of the evidence brought forward by the other. As a result an almost identical argument woven into both papers is used to support opposing points of view. It is based on experiments in which the percept is so compellingly determined by some aspects of the 'field' that it can hardly be modified, even when the subjects know that the physical arrangements of the experiment are not what they appear to be. For Michotte, who refers to his work on the perception of causality, this demonstrates the importance of 'pre-established structures', independent of experience and knowledge. Piéron, who describes the well-known experiments showing the effects of 'object-qualities', draws a generalized conclusion in favour of past experience.

The role of motivation in perception is discussed in the papers by Ombredane and Nuttin. Ombredane's contribution is an eclectic and not very explicit attempt to bring together some concepts from the ex-'New Look' and information theory. Nuttin's paper deals with the perception of success and failure. It is a very useful summary of the experiments and theoretical discussions included in his recently published large volume on *Tâche, réussite, et échec. Théorie de la conduite humaine.*

The remaining two papers offer interesting accounts of experimental work, but they are hardly related to the central theme of the symposium.

H. TAJFEL

The Practice of Modern Perfumery. By PAUL JELLINEK (translated from the German by A. J. KRAJKEMAN). London: Leonard Hill. 1954. Pp. x+219. 25*s.*

Basic Odor Research Correlation. By R. W. MINER (ed.). *Annals of the New York Academy of Sciences*, volume 58, art. 2, 1954. Pp. 13–260. 24*s.* 6*d.*

These two books are important contributions to a comparatively neglected area of study. *The Practice of Modern Perfumery* contains the sort of 'inside information' which is rarely made readily available. It is a book by a practising expert. The first and last of its four (roughly equal) sections are rich in information and interesting hypotheses relevant to psychology. The first section includes instruction on how to devise and to carry out a 'suitability test' for those who might wish to become perfumers. The technique of 'olfactory analysis', which makes use of the well-known phenomena of olfactory fatigue, is described in sufficient detail for those interested to attempt such an analysis for themselves. However, the roles of training and experience are also made explicitly clear. The last section of the book is broader in scope and may be of wider interest to psychologists than the first. It is entitled, 'Perfumery, cosmetics and psychology'. The role of natural (clean) body odour as a source of sexual stimulation is discussed at some length, with supporting evidence. The author suggests that certain odours of an animal type produce pleasurable associations providing the 'intensity' of the odour is too low for it to be identified as having direct animal associations. He stresses the importance of visual associations. For instance, he quotes evidence to indicate that certain floral perfumes are unpleasant except when they are *directly* associated with the relevant flowers. He describes a four-fold system for classifying odours contrasting 'Erogenic' (sex-stimulating) with 'Anti-erogenic' (refreshing and disinfectant), and 'Narcotic' (intoxicating) with 'Stimulating' (activating, exciting). Later, the relation between perfume and personality is examined in this framework.

These are just a few of the many items of interest to psychologists. The experimental psychologist will find technical information of immediate value. The student of personality and the clinical psychologist will also find much to interest him. Observations on perfume and fashion would interest the social psychologist. In short, it seems reasonable to claim that, in the first and last sections of this book, there is something for everyone concerned with contemporary psychology.

The second book is a collection of papers by some forty specialists brought together under the title of *Basic Odor Research Correlation*. Bernice M. Wenzell writes on 'Problems of odor research from the standpoint of the psychologist'. R. W. Moncrieff (the only British contributor) writes on 'The odorants'. Jean F. Mateson writes on 'The olfactory area and the olfactory receptor process'; M. M. Mozell and Carl Pfaffman on 'The afferent neural processes in odor perception'; and K. H. Pibram and L. Kruger on 'The functions of the "Olfactory brain"'. R. M. Hainer *et al.* supply a stimulating paper entitled 'An information theory of olfaction'. This selection is essentially a personal one and in no way implies that the other contributions are less interesting or less important. In a speculative introduction A. R. Behnke outlines what the state of affairs in odour research might be in 1983, thirty years after the meeting organized by the American Society of Heating and Ventilating Engineers, at which these varied papers were given. At first sight this somewhat facetious approach might seem disturbing in a serious scientific publication. However, it reflects precisely the general impression which emerges from reading this collection of papers. New techniques are being developed and new knowledge, which seems to be arising as a result of those techniques, suggests that much that is now accepted and written into the *few* pages devoted to the sense of smell in most familiar text-books is likely to be drastically revised and greatly extended in the next two decades. This, like Jellinek's book, can be recommended as an essential addition to the psychological reference library. Those who have more of a specialist interest will no doubt want their own copies.

ROLAND HARPER

Manuel Pratique de Psychologie Expérimentale. By PAUL FRAISSE. Paris: Presses Universitaires de France. 1956. Pp. xi+312. 1.200 fr.

After a lucid, thirty-nine page discussion of the nature of psychological experimentation, this book presents sixty exercises for use in an introductory course in experimental psychology. For French psychologists it supplies a definite need since, according to Fraisse, no other such manual exists in their language. British psychologists who are concerned with organizing practical laboratory courses are more fortunate in having several fairly up-to-date guides at their disposal. For them, this manual contains little that is new, unless it be the inclusion of a small number of Michotte's demonstrations of the perception of causality. This book is a reminder that a really satisfactory set of laboratory demonstrations has not yet appeared. It seems to the reviewer that, in Britain alone, psychology departments could collaborate to provide a core of standard experiments for which no apology need be made to the student on behalf of this or that inadequacy and for which the accumulated results of many student subjects would be available. This might do more than anything else to engender in the novice a respect for and an understanding of experimental method.

I. M. L. HUNTER

The Evolution and Growth of Human Behavior. By N. L. MUNN. Boston and New York: Houghton Mifflin Co. 1955. Pp. x+525. 35*s*.

Teachers of psychology have frequently to contend with the problem that members of their classes approach the subject with very different backgrounds and frequently with no background at all in the biological sciences of which genetics, and some orientation in regard to phylogenetic development, are especially helpful. Dr N. L. Munn's *Psychological Development* (Houghton Mifflin Co., 1938) was in this respect a particularly useful book which provided a commendable amount of information, a wealth of reference and some clear introductions to fundamental issues. One recalls, for example, the discussion of the James–Lange and the thalamic theories of emotion. Unfortunately, the book was in recent years unobtainable in this country. The present work is intended to replace it.

There are many points of similarity between the two books, since much of the material and illustration in the earlier work is still relevant. The new work maintains the high quality of printing, paper and binding, but the page is larger (7 × 9½ in.), and the text is arranged in two columns. The sequence of topics is broadly the same as in the earlier work, but the treatment has been extended to include more recent experimental results. The seventeen chapters include those on 'The nature of genetic or developmental psychology', 'Unlearned behavior', 'Prenatal behavior', 'Basic factors in the behavioral development of children', 'Development of spatially coordinated behavior', 'Motor development', 'Language', 'The growth of intelligence', 'Emotional development', 'Development of social behavior' and 'Changes in personality from adolescence until senescence'. Naturally the wide range of reference has required some selection of the studies used, and the author's claim to have done this eclectically is justified. The exposition is consistently clear. The book could be read by the student very early in his university course. With its ample references and suggestions for further reading, together with extensive indices of subjects and of authors, it provides a clear and broad introduction to the vast literature associated with the concept of development. The impression does arise that the treatment is less intensive than in *Psychological Development*; but those who seek more are provided with ample and specific further references.

F. V. SMITH

Der Ausdrucksgehalt des menschlichen Ganges. By GERTRAUD KIETZ. Leipzig: Johann Ambrosius Barth/Verlag. 1956. Pp. 174. DM. 10.20.

Manner of walking as an expressive movement has always attracted much attention on the Continent, and this monograph contains a good survey of the literature. It also attempts to add to our knowledge by means of what the author mistakenly calls 'a well-designed experiment'. The idea apparently was to compare certain personality traits of her subjects with their behaviour in a situation in which they had to move from one place to another along predetermined lines. The results of this 'experiment' are impossible to evaluate because of the small number of subjects, the subjectivity of the appraisal, the invalid nature of the measuring instruments and the failure to treat the results in any statistical way whatsoever. The author, although quite knowledgeable as far as the Continental literature is concerned, does not give a single reference in English, and is apparently ignorant of the important work done in this field by such writers as Allport and Vernon. Had she been familiar with their twenty-years old *Studies in Expressive Movements*, her 'experiment' would not have been as inconclusive as it was.

H. J. EYSENCK

Gestalt Therapy: Excitement and Growth in the Human Personality. By FREDERICK S. PERLS, RALPH F. HEFFERLINE and PAUL GOODMAN. London: Pitman. 1952. Pp. xiii+466. 45*s.*

This book attempts to lay the foundation for the 'full application of Gestaltism in psychotherapy...to develop a theory and method that would extend the limits and applicability of psychotherapy'. The reader is invited to do eighteen experiments with himself; these are arranged in groups, 'orienting the self, and manipulating the self'.

Accompanying the detailed instructions and discussion of these 'experiments' are the comments selected from 100 undergraduates who did these experiments as homework.

The second part of the book discusses 'basic neurotic dichotomies of theory', body-mind, self and external world, etc., and leads up to 'a theory of self and creative action'. There is a critique of psychoanalytic theories, and recognition of the influence of Korzybski, Reik and non-orthodox analysts.

The book is too long. The practising psychotherapist may, according to his school, derive some stimulus from it. The academic psychologist is more likely to want answers to more basic questions before taking time off for it. The book has no index.

J. C. KENNA

The Great Mother: An Analysis of the Archetype. By ERICH NEUMANN. Translated from the German by RALPH MANHEIM. London: Routledge and Kegan Paul. 1955. Pp. xliii+380+185 plates. 50*s.*

There can be no doubt that Erich Neumann, German born, working in Tel Aviv, is one of the most creative and original analytical psychologists. *The Great Mother* is suitably dedicated 'To C. G. Jung, Friend and Master in his Eightieth Year', and the admirers of Neumann's earlier works, especially of the already classic *Origins and History of Consciousness* (1954), will not be disappointed by this latest creation. His style is lucid and elegant in its perfect translation, and Neumann knows how to write a psychological dissertation which at the same time is an aesthetic feast and a learned tome. The anthropologist, even if he may not always agree with the author's deductions, will find much of special value in the vast material presented, while the artist and art student may be deeply interested in Neumann's interpretation of so many works belonging to the most various ages and cultural spheres. The analytical psychologist, however, will be the one to profit most from this clear exposition of archetypal life in the psyche of man. 'What is an archetype?' ask the anti-Jungians somewhat disdainfully; I think that even the most stubborn resistance against the understanding of this conception of analytical psychology is bound to crumble in the face of this masterly explanation.

Apart from the elaboration of this Jungian principle the author adheres strictly to the task of investigating the particular archetype of the Great Mother. In doing this he works throughout with only a very few fundamental ideas regarding its nature which he explains at the outset. He faithfully carries out his research and resists the temptation of being drawn into the discussion of side-issues. Diagrams help to give the greatest possible clarity to the strategy of his working method; and, whatever one may think about diagrams in general, these are really helpful and further reveal Neumann's most original mind. Special praise is due to the way in which these 'schemata' can be unfolded and kept before the reader's eye while he studies the text relevant to them. The reproduction of the many illustrations is of the highest technical and artistic standard, and the publishers have, indeed, succeeded in producing a most valuable and beautiful book.

W. P. KRAEMER

Individuation: A Study of the Depth-psychology of Jung. By JOSEPH GOLDBRUNNER. London: Hollis and Carter. 1955. Pp. viii+204. 21*s.*

This work has been translated from the German by Stanley Godman, and is divided into two parts. The first part deals with Jung's approach to psychology and the problems of personality, neurosis, the unconscious, the treatment of psychological disorders, the archetypes and religious experience. This occupies

156 pages and, as an exposition of Jung's ideas, it is about as clear as would be possible. The second part is much shorter, and is devoted to an examination of the bearings of Jungian psychology on religion, anthropology, ethics, education and the cure of souls. Many of the points set forth in this part of the book will be of considerable interest to students of Jung.

R. W. PICKFORD

Studien zur analytischen Psychologie. Two vols. Edited by the C. G. Jung Institut, Zurich. Zurich: Rascher Verlag. Vol. 1, pp. xi + 396. Vol. 2, pp. ix + 397. Swiss francs 22.90 each vol.

When Prof. Jung celebrated his eightieth birthday last year, the Zurich Institute which bears his name presented him with these two volumes of collected articles. Their various authors have all, at one time or other, lectured at the Institute which, in the seven years of its existence, has faithfully served the aim of its founders to act as a centre of teaching and research. In the introduction by the Board of Governors, it says of the authors: 'Interest in analytical psychology and gratitude for everything that they have received from Jung is common to all of them.' In spite of the great difference in background of the thirty-two contributors, this cross-section of the work done at the Institute tells, in fact, a vivid tale of the harmonious growth of a new attitude, based on the *Weltanschauung* which is connected with Jung's name.

The first volume contains 'contributions to theory and practice'. The second is devoted to the history of civilization.

While it is not possible to give an individual appreciation of the articles here, I should like to mention that Mrs Jung, whose recent death has saddened us, is one of the authors, and that her contribution on the Anima is a most beautiful and sensitive fruit of her wisdom and knowledge. She, of course, like most of the authors of the 'Studies', writes in German, but there are also a few short French essays and a considerable number written in English. As the latter may be of particular interest to British psychologists, I mention some of their writers by name: E. A. Bennet, whose casuistical sketch 'The Double' is particularly interesting, is one of Jung's oldest friends. Michael Fordham and Gerhard Adler are both leading figures in the London Society of Analytical Psychology. Father Victor White of Oxford and Frances G. Wickes of New York have for many years been most creative interpreters of Jung's work. But there is no space to mention many other equally distinguished names. There is E. Neumann of Tel Aviv, for instance, whose general contribution to analytical psychology is quite outstanding and whose article is of the very highest quality, or K. Binswanger, B. Hannah, J. Jacobi, M. L. v. Franz, who all belong to the circle of Jung's closest collaborators.

The books make fascinating reading. Many of the papers will be of equal interest to the general reader, who has an interest in 'the new attitude', and to the specialized psychological research worker and the analytical psychotherapist. Jung's movement is so fully alive and of such dynamic power that a study of this work is certain to stimulate the 'analysed' and 'unanalysed' alike.

W. P. KRAEMER

Narcissus: A Psychological Study of Self Love. By GRACE STUART. London: Allen and Unwin. 1956. Pp. 166. 12*s*. 6*d*.

This is a short and very readable, but rather diffuse, book in which psychoanalytic ideas combining strictly Freudian concepts with those of Melanie Klein, Fairbairn and others are set forth and applied. The book has literary merit and is illuminated with many apt quotations from poetry, plays and other works as well as showing psychoanalytic erudition. The author is to be commended for a clear grasp of the problems of early infancy from the point of view of the infant, and narcissism is understood and presented with this insight. The earliest infantile relation with the object is one of identification, and narcissism is not only a phallic level problem, but very much more of pre-genital and indeed of early oral origin. If the book helps some of the public interested in psychoanalysis to grasp more clearly these points, which are so painfully obscure to many people, then it will do a signal service.

R. W. PICKFORD

The Psychology of Economics. By WALTER A. WEISSKOPF. London: Routledge and Kegan Paul. 1955. Pp. viii + 266. 25*s*.

This book is concerned with an analysis of factors which influence the choice and retention of economic concepts. The questions of bias in economics, and of the place and influence of value judgements, have occupied social scientists for many years. Weisskopf, in stressing the existence of unconscious bias, approaches the problem from a psychoanalytic angle, and is probably one of the first writers to claim that bias in the selection of economic concepts fulfils an important psychological function.

Weisskopf sets out 'to explain the function and meaning of some economic ideas with the help of the psycho-cultural method'. He holds that economic thought must be viewed in the context of psychological conflicts which are caused by cultural and socio-economic conditions. Thought-constructs must be considered as a reaction to anxiety-creating conflicts and a way of dealing with them. In uncovering the unconscious function of thought, Weisskopf goes beyond psychoanalysis by postulating that 'basic human conflicts are not only of a biopsychological but also of a social, intellectual, and spiritual nature'. Individuals share many conflicts with their contemporaries, and systems of socio-economic thought provide solutions of supra-individual social importance.

In a series of well-documented case-studies, Weisskopf examines the value theories of Adam Smith, Ricardo and Marshall in the light of conflicts in the 'value-complexes' of their times. In the most provocative part of the book, he discusses male and female symbolism in the thought of Ricardo, Malthus, Engels and Marx. Many of the arguments about symbolism appear far-fetched; the author is more convincing where he deals with the influence of factors other than biopsychological ones, and it would be a grave mistake to reject all his arguments, because some are unconvincing. The explanations of contradictions in Marshall, for instance, are very striking.

This book raises important issues which should be of interest not only to economists but to all social scientists. Parts I and VI are probably of greatest interest to non-economists.

HILDE BEHREND

Guide du Psychotechnicien. By C. G. FONTAINE. Paris: Dunod. 1956. Pp. 119. No price given.

The interest of this little book for British psychologists does not lie in the chapters which aim at advising the intending psychologist or informing the lay public. It lies in the veritable mine of information about the status, the formation, the prospects and the worries of psychologists in France. A university degree in psychology is a recent development (it was instituted at the Sorbonne in 1948), and prospects for graduates are not too bright. Training in some applied fields (e.g. vocational guidance and selection) has reached high standards; in others, such as clinical and educational psychology, it lags behind considerably. Conditions of employment are often ill-defined.

An interesting account of public reactions to the practice of psychology is given. It seems that some employers stand in urgent need of the guidance provided by this book; an industrialist was recently seeking a psychologist 'who would also be capable of administering first aid and dealing with the wages of the personnel'. In the trade unions, the tradition of resistance to Taylorism has largely carried over to attitudes towards industrial psychology, but there are some hopeful signs of change. The medical profession insists on adequate scientific standards. These include a firm dismissal from psychological assessments of any tests of 'psychoanalytic nature'.

The author feels that legal and ethical codification would help a great deal. There is an appendix containing detailed suggestions for an ethical code. This should be found topical by many.

H. TAJFEL

Leadership Qualities: a Theoretical Inquiry and an Experimental Study on Foremen. By KULLERVO RAINIO. Helsinki: The Finnish Academy of Science and Letters. 1955. Pp. 211. 750 marks.

Since its foundation in 1947, the Institute of Industrial Administration of Finland has been concerned with training in leadership skills in industry and, inevitably, with selection. This book is an interim report of research in the selection of foremen in which 700 industrial supervisors were tested and twenty-eight different tests explored, ranging from intelligence, through group discussion to Rorschach.

To find a way among the many personality traits that have variously been claimed to be needed by leaders, and bearing in mind the argument that the group structure rather than individual personality is critical, the author starts by making a theoretical analysis of 'leadership' as such. This he does in Lewin's topological terms, and distinguishes four conditions of interrelationship between the forces in the social field necessary for 'leadership' to occur. Stepping across now, a little anxiously, into non-topological terms he hypothesizes four personality characteristics, or 'leadership qualities', that will 'on the average most often make possible fulfilment' of these relational conditions. In the light of his analysis he labels these non-suggestibility, energy, suggestivity of personality or authority, and maturity or ability to maintain direction in a conflict situation; intelligence appears to be of minor importance.

Tests were then given which offered assessments of these traits, although tests used in other countries for the selection of supervisors were also explored. Four criteria of leadership were used: a rating by superior for 175 of the men, a rating by Institute instructors for 22, a 'sociometric' rating by fellow-students for 117, and a criterion based on the efficiency of each foreman's shop for 79. There was a slight overlap; three different criteria were obtained for 22 of the men, and two for something under 38 others.

Five factor analyses were made, and the results combined by Ahmavaara's transformation analysis. Four factors were discerned and identified as general intelligence, non-suggestibility, stability and a problematical one called activeness. The first three had significant value in assessing leadership qualities, intelligence having least value, so confirming three of the hypotheses. No confirmation was found of the 'authority' hypothesis, which is taken to suggest that 'authority' is an empirical consequence of the high level of other leadership factors. The hypothesis about 'energy' remained doubtful.

The loadings suggest a test battery of intelligence and arithmetic tests for factor 1, body sway and File's 'How Supervise?' for factor 2, and leaderless group discussion, Maudsley Medical Questionnaire as dichotomous, and an intriguing distraction test for factor 3. These give a theoretical multiple correlation of +0·59, with rating by superior, and in a try-out on 26 subjects a correlation of +0·42 ± 0·17 was obtained.

The results of the study could be important. The group task test again comes up strongly, correlating +0·38 with rating by superior, and again it is shown that evaluations by different observers correlated as highly as +0·7. Other results are less convenient. The battery is a long one—two hours of group tests, two hours of tests in small groups, and twenty minutes of individual tests. Yet in a N.I.I.P. study a single intelligence test has given as high a correlation with supervisory performance as does this whole battery. More important, Rainio's correlation between intelligence and rating by superior was as low as +0·08, although other studies have found correlations ranging up to +0·6.

This makes us look again at the present study, when it appears that the criterion of rating by superior is of doubtful value. This was obtained by questionnaire (apparently to be returned by the foreman himself), and cross-checking suggests that the 175 completed were out of some 300 sent out; moreover, the ratings were by different raters, in different firms. In the end, most of the foremen reported on were rated as in the average to above average area, 4 points of the 9-point scale covering 86% of them.

Another point is that different groups of subjects did largely different tests, although it is claimed that since the groups consisted of foremen gathered at random there is no significant difference between them. The sort of combining of results involved appears when one examines closely which groups did which tests. Looking at the tests suggested for the battery it turns out that of the 700 men tested only 63 actually did all of them. For 10 of these men there was no criterion assessment at all, for the other 53 there was the sociometric rating, and for fewer than 16 of this 53 there was also the rating by superior. Looking at the intelligence test results in this way, correlations are quoted of +0·31 with group task ($N = 442$), +0·25 with sociometric rating ($N = 117$), and +0·08 with rating by superior ($N = 175$); but in fact only 38 subjects could have been common to all these samples, or even to the last two.

Clearly a tremendous amount of exploratory work has been done and a review cannot do justice to it all. Yet doubts about the criterion and about the combining of results make one feel that two or three much smaller scale studies, each complete in itself and each in the context of a single firm, might have carried more conviction even if they involved small samples. Moreover, the author himself argues that the conditions of leadership are relational, citing, for instance, that the intelligence a leader needs is related to the intelligence of the people he leads; yet his approach is one that is bound to swamp out this kind of point. None the less, he has made a worth-while attempt to put his research into a wider theoretical framework, although the combination of topological psychology and factorial analysis, all in translation from the Finnish, make solid fare for the reader.

R. M. MCKENZIE

The Presentation of Self in Everyday Life. By ERVING GOFFMAN. Edinburgh: University Social Sciences Research Centre. 1956. Pp. 162. 10*s*.

This book sets out a new way of looking at social interaction, using the analogy of the theatre. The author shows how a great deal of social behaviour can be regarded as an attempt to put on a show for an 'audience', in order to create in them certain perceptions of the 'performers'. He distinguishes between 'front regions', where the performance takes place, and 'back regions', where the team of performers can relax and crack jokes about the audience. This analogy is followed up with many ramifications, and illustrated from the sociology of the professions and commerce, anthropological studies, books of etiquette and literature. This book breaks all the rules—the evidence quoted is largely anecdotal, the terminology introduced is often vague and cumbersome, and testable hypotheses are not put forward, though they could be dug out. On the other hand, it is quite fascinating and, it may be said, most illuminating. This is partly because many of the examples are quite hilarious, partly because the whole point of view really does give increased insight into social situations.

It may be objected that this is a *tour de force*, and that the author has exaggerated the extent to which behaviour is motivated by the need to create certain impressions—it would be more usual to postulate a continuum extending from pure acting, through lesser forms of role-playing, to involuntary twitches at the other end. It may also be pointed out that many other needs are operating in social behaviour besides that of manufacturing perceptions about oneself. Nevertheless, the impact of the examples collected together in this book is to give a convincing demonstration that a great deal of social behaviour can be analysed in this way, and that this process is probably more widespread than has usually been thought.

MICHAEL ARGYLE

Family: Socialization and Interaction Process. By TALCOTT PARSONS and ROBERT F. BALES. London: Routledge and Kegan Paul. 1956. Pp. xvii + 422. 30*s.*

This book consists of variations on the theme of the family treated as a small group. In an introductory chapter, Parsons looks at the contemporary American family and argues convincingly that the high divorce rate does not necessarily indicate a progressive disorganization of the family as an institution; he believes it is the result of strains involved in changes in the roles of the marriage partners, changes that will in the long run actually strengthen the marriage ties.

In the next three chapters, the process of socialization is examined in a manner somewhat unfamiliar to psychologists, though readers of Parsons's previous works will not be entirely unprepared. As far as the reviewer understands, Parsons postulates a basic parallel between what he calls a 'personality system' and a 'social system'; it follows that the method of analysis used for the latter will also be appropriate for the study of the 'personality system'. Hence the development of the child is viewed as the progressively more complex internalization of 'patterned action systems', i.e. roles in relation to the other members of the nuclear family. Roles within the family are classified into various types, and Parsons holds that the child's learning proceeds by 'binary fission', analogous to cell division in organisms; thereby the original simple 'mother-child system' becomes differentiated to include the father and siblings, first in relation to their intra-familial roles only, and later in relation to their roles in the wider social structure. The following illustration will show that this is a gross over-simplification of Parsons's position as formulated in his characteristic style:

'However, we may suggest that the step from four to eight consists in a bifurcation of the instrumental-expressive category by a cross-cutting differentiation of universalistic and particularistic aspects. The bifurcation on the instrumental side produces a distinction which has sometimes been referred to as the difference between technical and "executive" roles.

'The parallel differentiation on the expressive side is that between integrative functions which are primarily particularistic, and pattern-maintenance-tension-management functions which are primarily universalistic in focus.'

The main lesson for psychologists which he drives home repeatedly is that biological needs or drives '*cannot* serve as the primary basis for the organization of personality as a system'. They are limiting, not determining factors; organization is brought about by social interaction and implies control over organic needs. One of Parsons's few critiques of psychoanalytic theory concerns its model of 'a "linear" branching system of motivational strands which then operate independently of one another'. Otherwise his stages of role-differentiation are closely parallel to the Freudian stages of psycho-sexual development. Perhaps it would be legitimate to say that he has in fact translated the somewhat loose psychoanalytic account of child development into the rigid framework of a sociological role-theory. In the process all the life has inevitably been drained out of it; the question is of course whether any gain in scientific insight has resulted.

Before discussing this, some mention must be made of two other parts of this work. There is, first, a chapter by Bales and Slater on 'Role differentiation in small decision making groups'. It describes a series of experiments with college students which seem to have a very slender bearing on the central topic of the book. The main outcome of these investigations was to demonstrate that role differentiation did occur in these groups. The hypothesis emerges that 'the tendency toward differentiation must arise out of factors associated with social interaction over a very broad range of situations'.

There is also a carefully executed and cautiously presented comparative study by Morris Zelditch. It deals with role differentiation within the nuclear family in a variety of different cultures. This lends some support to Parsons's claim that the division of intrafamiliar roles he describes represents a widespread, if not universal pattern. Zelditch suggests that variations in parental roles may be a function of the relationship of the nuclear family to the wider descent groups.

What is liable to strike the psychologist most, and probably unfavourably, in this book is Parsons's almost complete lack of reference to concrete pieces of behaviour. We are confronted with a formal hypothetical system at a high level of abstraction. This in itself is of course not a criticism; on the contrary, the social sciences are often accused of unbridled empiricism, the random collection of data unordered by any coherent theoretical framework. The crucial question is whether Parsons does in fact provide such a framework. For instance, is the Freudian theory of psycho-sexual stages more amenable to verification in its Parsonian form? Or can any other testable consequences be derived from his system? The reviewer does not feel confident of having a sufficient grasp of Parsons's thought to venture a definite answer. His own is perhaps a little disappointing:

'If our view is correct all the main pathological syndromes should be found in all human populations. Their relative incidence, however, would be expected to vary substantially...This should prove to be a testable claim.'

Although one may suspect that the attempt at a synthesis in the grand manner is premature, the work has much to offer with regard to specific problems. An example is Parsons and Olds's treatment of such topics as internalization or frustration, which is original and thought-provoking. A good deal can be learnt by looking at familiar problems from an unfamiliar angle—various aspects become apparent that one had previously missed. This will be the reward for psychologists with sufficient perseverance to struggle through this book.

One last complaint: there seem to be more than the usual quota of printing errors, and page 352 is blank.

G. JAHODA

Democracy and Dictatorship: Their Psychology and Patterns of Life. By ZEVEDEI BARBU. London: Routledge and Kegan Paul Ltd. 1956. Pp. viii + 275. 28*s*.

Dr Barbu's book is the latest attempt to describe and analyse the characteristic habits of response in democratic and totalitarian countries. He has avoided most of the errors of his predecessors, including that of over-stressing patterns of child care, and has endeavoured to do justice to economic, sociological and historical factors. Indeed, the author's background as a European academic and diplomatist who has had first-hand experience of both German and Russian forms of totalitarianism gives weight to his psycho-social analysis and provides it with a balance often lacking in other studies in this field.

The book is in three parts, treating respectively the psychology of Democracy, Nazism and Communism. In each case a few characteristic features are singled out for detailed study. Thus, for instance, the processes of social and cultural flexibility and the rationalization and minimization of power are taken as characteristic of all democratic societies, whilst processes of social and cultural rigidity, and the emotionalization (Nazi) or super-rationalization (Communist) of life together with the concentration of power are characteristic of modern totalitarian societies. Perhaps two of the most interesting sections of the book are the discussions of the replacement of religion by dogma and the pattern of individuation in communist societies, the arguments here being both subtle and convincing.

Two criticisms may be made. First, Dr Barbu's attempt to discuss the ideal-type personality structure of the communist with reference to the personalities of Marx, Lenin and Stalin, fails to strengthen an argument which does not depend on it. Secondly, his very close identification of the democratic pattern of life with Britain is generous rather than realistic; it ignores various authoritarian and rigid elements easily discernible in schools and industry.

In general, this book is most readable and one of the best in the literature of its kind.

DUNCAN MITCHELL

To Define True Madness. By HENRY YELLOWLEES. London: Pelican. 1955. Pp. 172. 2*s.* 6*d.*

This book can be read with enjoyment and profit by anyone who is interested in medical psychology. Much of the raw material will be familiar, but it is presented here in a lively and original manner. Dr Yellowlees, well versed in the Bible, Shakespeare and English literature generally, is an excellent writer, always lucid and forceful, and often witty. An expert at quoting aptly, he often writes quotable prose himself: 'You should talk to the insane, generally speaking, as if you were talking to the sane. You can never go far wrong if you can do that, provided that your talk to the sane is worth hearing.'

His discussion of insanity as a defence in law is important, for he has more practical experience of this problem than most of his colleagues. 'It is impossible to give a clear or accurate clinical picture of mental disorder in terms of the (McNaghten) rules, because their terms are those of a conception of mental disorder which has long ago been abandoned, if, indeed, it was ever seriously held.' In actual practice the McNaghten rules work 'because they are an almost perfect example of that illogical, commonsense, tacit compromise to which the law constantly resorts when it is faced, as it is here, with an insoluble problem'. In the author's opinion, the question of fitness to plead should be given more attention at present than the McNaghten rules.

In a chapter on 'The bad old psychiatry' which Dr Yellowlees recalls with respect and affection, he includes some charming reminiscences of his earliest dealings with the insane in a mental hospital of which his father was medical superintendent.

He believes that mental hospital experience is necessary for the psychiatrist, and he thinks that the art of healing is as important as the technique, and that its essence lies in the personal relationship between doctor and patient. In his view, this art is at present in decline because psychiatry has come to depend too much on 'its amazing technique'. He vigorously criticizes the psychiatrists who exemplify this development, and he considers that the training of psychiatrists today is not all it should be.

These views will be read with great interest by workers in this field. But was the author wise to deal with these topics in a book meant for laymen? This Pelican will probably be read by thousands of people including sick men and women who are looking for help, and, although they will learn something about psychotherapy and other methods of treatment, these prospective patients may wonder to whom they should turn. For it would not be surprising if such readers formed the impression that the only psychiatrists to be trusted are those with the same kind of training and experience as Dr Yellowlees.

The author acknowledges psychiatry's debt to Freud, and his own description of psychopathology is largely Freudian. He scorns official psychoanalysis however, is sceptical about its therapeutic methods and reserves his heaviest irony for those who have had what he calls a 'complete Freudian psychoanalysis'.

The views of so able and experienced a psychiatrist are well worth hearing, and although Dr Yellowlees is not always objective (and presumably would not claim to be) he is invariably entertaining.

V. B. KANTER

On Human Thinking. By K. W. MONSARRAT. London: Methuen. 1955. Pp. 156. 15*s.*

Though slight in bulk this is an ambitious work which endeavours through a philosophical examination of human thinking to correct man's social errors and pave the way for social harmony and happiness. Dr Monsarrat, a medical man, whose earlier writings on human powers and human desires showed him to be thoughtful and original, was not badly qualified for his task and his views, though somewhat abstruse, are suggestive and interesting. Briefly he believes that impersonal, objective or scientific knowledge contains *a priori* features derived from our direct personal experience of power in action and temporal endurance, and that these direct personal intuitions contribute to a dynamic understanding of the world without, and enable man to comprehend social systems as organized systems in dynamic equilibrium. Unfortunately, man has misread the lessons of his own experience, and the source of his mistakes lies deeply in habits of thought. The author examines some of the erroneous modes of human thinking, philosophical, religious and political, and points the way to a mode of thought which integrates the evidence provided by experience and by history. It is in the role that he gives to inner personal experience in the interpretation of external reality that Monsarrat is most challenging, and most likely to interest psychologists.

L. S. HEARNSHAW

The Harvard List of Books in Psychology. Second edition. Harvard: University Press; London: Geoffrey Cumberlege. 1955. Pp. viii + 84. 10*s.*

This list contains six hundred and seven books selected to represent all areas of contemporary psychology. The books mentioned are those which have appeared up to March 1954, and each is briefly annotated. Although no bibliography of this sort can avoid arbitrariness, its quality is guaranteed by the fact that the compilers are members of the Department of Psychology, the Department of Social Relations, and the School of Education in Harvard University. It will interest those concerned with maintaining psychology libraries and with preparing reading lists to accompany courses in almost any of the numerous branches of psychology.

I. M. L. HUNTER

Situation. Utrecht: Spectrum. 1954. Pp. 260. No price given.

Situation is a collection of 'Contributions to Phenomenological Psychology and Psychopathology'; it is due to appear once every year, this being the first volume. The title is somewhat misleading. The book does not contain an account of phenomenological experiments in perception and other fields, but is full of essays of a belletristic kind on such subjects as 'Marriage and Love', 'L'Expérience Du Vide', 'On Hope' and 'The Physiognomy of Prognostic Uncertainty'. It is doubtful if psychologists (in the technical sense) would find anything of interest in this trilingual review, although it might help them to appreciate the enormous difference between Anglo-American and European concepts of psychology.

H. J. EYSENCK

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A TWO-PART PERSONALITY MEASURE FOR USE AS A RESEARCH CRITERION

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The present paper describes the development and evaluation of a 20 min. two-part personality measure suitable for use as a research criterion in a wide variety of situations. One part provides a score of emotional maladjustment based on twenty of seventy-four items, having an internal consistency of 0·81; the other part a score of sociability based on twelve of thirty-six items, having an internal consistency of 0·74. Evidence is presented for the validity of both scores, and for their independence of age and intelligence as assessed in adequately heterogeneous samples.

I. INTRODUCTION

Psychological research is not always directed to the evaluation of individual differences in intelligence or temperament, but experienced workers constantly encounter situations where such evaluation is essential as a control in the selection of experimental subjects. It is therefore a customary precaution to obtain a measure of intelligence in case it may prove to be related to the variables under investigation, but all too often a similar precaution is omitted where emotional factors might be a further source of variance. Such an omission may often be due to the lack of a suitably brief, reliable and valid measure. The present paper describes the development of such a measure, which takes the form of a two-part inventory which has during a period of over four years been used widely under various conditions in industry and elsewhere. Publication has been withheld until the information concerning its characteristics was considered adequate to justify making it available for use as a criterion measure suited to the needs of research workers with a wide range of interests.

Most readers will be familiar with many of the questionnaires and inventories which have been published during the past forty years; by now it is hardly possible to produce a new item. Such inventories have been variously named, but in one way or another the majority can be described as concerned with emotional maladjustment, some aspect of 'sociability' or 'introversion-extraversion' coming a close second.

The first part of the present inventory is intended as a measure of emotional maladjustment; a determined effort has been made to provide in the second part a measure of sociability which is as far as possible independent of the first measure. As historically the development of the first part preceded that of the second, and provided evidence which is still relevant to an evaluation of the final product, a brief summary of this development stage is necessary at the outset.

II. DEVELOPMENT OF FIRST PART

The original draft consisted of seventy-four items, and this has been maintained as the total throughout the whole programme of development. This was made up of three types of item: thirty intended as indicators of emotional maladjustment when marked in the significant direction; thirty concerned with physical health, intended principally as

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buffers for the emotional items; and fourteen drawn from the so-called 'Lie Scale' of the Minnesota Multiphasic Personality Inventory (MMPI). The thirty-six items in the first group were drawn mainly from the Maudsley Medical Questionnaire (Eysenck, 1952) and from Winne's study (1951) of selected items from the MMPI, together with a few items from Cattell's factor analysis studies and from U.S. Army questionnaires. All items found in the form of questions were re-cast as statements, some were altered from positive to negative form or vice versa, and quite a few had to be modified slightly because of unfamiliar American idioms.

In this original form the inventory was used in a large-scale study of unskilled male and female workers in electrical engineering factories. The samples involved consisted of men ($N=100$) and women (N's$=100$ and 124) between the ages of 18 and 60. No significant difference was found for emotional maladjustment scores between male and female samples, and all correlations with age and intelligence were statistically insignificant. Although every one of the original thirty items had been shown by the source authors to distinguish significantly between groups of 'neurotic' and of 'normal' respondents, it seemed desirable to obtain evidence of internal and external validity for the total score when used in the existing circumstances and in company with the buffer items referred to. In one sample of 124 women who had been subjects in a study involving a large number of personality tests (Heron, 1955*b*) the thirty-item total score obtained the highest loading (0·64 after rotation) on a factor readily identifiable as emotional instability or maladjustment. In this study the split-half (odd-even) self-correlation, corrected to full length, was 0·82. When the distribution of scores for fifty female neurotic in-patients was compared with that for a group of twenty-one female unskilled workers passed by a psychiatrist colleague as 'apparently free from signs of neurotic illness', only 12% overlap was found. It therefore seemed worth while continuing to use the full seventy-four-item inventory, while contemplating the possibility of re-keying the score for emotional maladjustment in the light of fresh data and further analysis. The only alteration of actual items was therefore carried out at this stage, six items being dropped (four from the 'Lie Scale' and two from the 'physical health' group), all of which were replaced by six fresh items likely to measure emotional maladjustment, thus raising the total score for this to thirty-six.

In this form, not subsequently altered, the inventory was employed in a fresh study of 214 new entrants as male omnibus conductors (Heron, 1955*a*; Venables, 1953). The split-half self-correlation, corrected to full length, proved to be 0·83 whether one used odd *v.* even items or first half *v.* second half as the basis of calculation. Correlations with age and intelligence remained insignificant, and the loading in a large heterogeneous battery on a factor of emotional maladjustment was again the highest at 0·53. Overlap of distributions between seventy-five symptom-free men and fifty neurotic in-patients was 16%. It was possible to obtain re-test data for seventy-eight of the original 214 men after an interval of 6 months. As these seventy-eight were of course among those who had not resigned or been discharged from the job, the re-test correlation was likely to be shrunk due to restriction of range if emotional maladjustment scores happened to be related to leaving behaviour. This proved to be the case, and accordingly the obtained coefficient was corrected by means of the two variances concerned, giving a final estimate of 0·75. It will be noticed that this does not differ greatly from the split-half correlation of 0·83, suggesting that 'function fluctuation' is relatively slight.

Before describing final development work on the first part of the inventory, it will be necessary to describe the development of the second part which took place at this stage.

III. Development of second part*

The item-source for a measure of sociability used on this occasion was Guilford's '*R*' (rhathymia) scale. On the basis of material obtained by Hildebrand (1953) the seventy-one items were analysed for frequency of response on the part of two contrasting groups of hospital patients, twenty-five diagnosed as hysteric, twenty-five as anxiety states. After elimination of items too American in idiom for general use, thirty-six were selected which best discriminated between the two groups. It will be observed that an assumption was made here in order to overcome the apparently insoluble problem of obtaining criterion groups of extreme 'introverts' and extreme 'extraverts' in the general population. The assumption is that a man diagnosed as an 'hysteric' is more likely to be a neurotic 'extravert' than a neurotic 'introvert', and contrariwise for anxiety states. This assumption finds support in the work summarized by Eysenck (1952, 1953). The term 'sociability' has been selected for this part of the present inventory mainly because it is somewhat less likely than is 'introversion-extraversion' to be narrowly identified with any particular school of thought in contemporary psychology.

These thirty-six items were then used as a second part of the inventory, administered after an interval during which some other test or activity was inserted. The need for such a break was found to be generally desirable from observations made during the administration of the seventy-four-item first part to several hundred industrial respondents of both sexes. No complaint about length was encountered earlier than the sixtieth item, but there were a few thereafter, suggesting that the seventy-four-item length represented something of a maximum if sustained interest were to be obtained from a majority of respondents unused to such procedures.

IV. Final analysis of whole inventory

For the purpose of final selection of items to represent emotional maladjustment and sociability respectively, the writer waited until it was possible to accumulate an adequate sample. This was finally made up as shown in Table 1.

Table 1. *Composition of main development sample*

Omnibus conductor applicants,† accepted for employment, 'helping research'	100
Army (National Service men)	118
University students	19
Police drivers and instructors	14
Total, 'normals', research conditions	251
Omnibus conductor applicants,† inventory forming part of medical dept. examination before acceptance	100
Total, 'normals', varied conditions	351
Neurotic in-patients, mixed diagnoses†	27
Total	378

† Not part of the previous studies—all fresh samples.

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* All the early development work on the second part of the inventory was carried out by the writer's colleague, Dr Peter Venables, who was also responsible for supervising the first-stage item analysis described in the next section of this report.

For this sample of 378 subjects, two sets of item analyses were carried out. These involved all 110 items (seventy-four plus thirty-six). The 378 sets of responses were placed in order according to total score for emotional maladjustment, and then divided into lower, middle and upper thirds. A count was then made of the number of times each item had been answered in the significant direction within each of these thirds. An item making a useful contribution to the total score would be expected to show a small total in the lower third and a large total in the upper third. If it were also discriminating in a perfectly linear fashion, the magnitude of the total for the middle third would be exactly intermediate, e.g. 20–40–60, 40–65–90. Using the two-fold requirement of linear discrimination and highly significant contribution to the original total score, twelve items were selected as a new total score to serve as the basis for a fresh item analysis.

A similar first-stage analysis was also carried out, again using all 110 items, in respect of the original thirty-six-item total score for sociability. In the selection of items for the second-stage analysis, a further requirement was added, namely, that an item selected to represent sociability should have made little or no contribution to the original total score for emotional maladjustment. On the basis of all three criteria (usefulness, linearity, independence), twelve items were selected as a new total score for sociability in the second analysis.

At the conclusion of the second-item analysis using the twelve-item total score for emotional maladjustment, it was found possible to select twenty items in all as a final total score. This twenty-item total score was then used as the criterion of emotional maladjustment when attempting to find further useful, linear and independent items to measure sociability. The stringency of this three-fold criterion, however, resulted in failure to produce a greater number of items, and the final total score for sociability had consequently to be based on twelve items.

To recapitulate: the twenty and twelve items finally chosen to represent emotional maladjustment and sociability respectively were those which had (*a*) shown a statistically significant contribution to the appropriate total score, (*b*) made this contribution in as linear a fashion as possible, and (*c*) as far as possible were *not* contributing significantly to the total score for emotional maladjustment when they were supposed to be measuring sociability.

V. DESCRIPTION AND EVALUATION OF THE PRESENT TWO-PART MEASURE

This two-part personality measure consists of 110 statements which can be answered as 'True' or 'Not true' self-descriptions by the subject. The first part, seventy-four items, contains twenty items which are used as the basis for a score of emotional maladjustment. Apart from eight items of the MMPI 'Lie Scale', which can be used as a separate score if desired, most of the remaining items are buffers which cause the section as a whole to appear as a 'Health Inventory'. Average response time for this section is about 12 min. The odd-even self-correlation of the twenty-item score for emotional maladjustment, corrected to full length, is 0·81 ($N=378$). Each item discriminates significantly at the 5% level, or better, between lower, middle and upper thirds of total score, when χ^2 on observed differences from chance expectancy is used.

As three-quarters of these items discriminate more sharply between middle and upper thirds than between lower and middle thirds of total score, despite efforts to find strictly linear items, the distribution of scores shown in Fig. 1 is not surprising. It will be observed that scores up to 9 are distributed in a roughly normal fashion, and that there is a 'tail' from 10 upwards. One standard deviation above the mean for 251 'normals' falls at 9·03, one standard deviation below the mean for twenty-seven hospitalized neurotics falls at 9·48. It seems justifiable to regard scores of 10 and above as 'probably maladjusted'; scores from 0 to 7 as 'probably well adjusted', and to reserve scores 8 and 9 as 'doubtful'. The use of these groupings on the 251 'normal' respondents would divide them as to 13% 'probably maladjusted', 13% 'doubtful', and 74% 'probably well adjusted'.

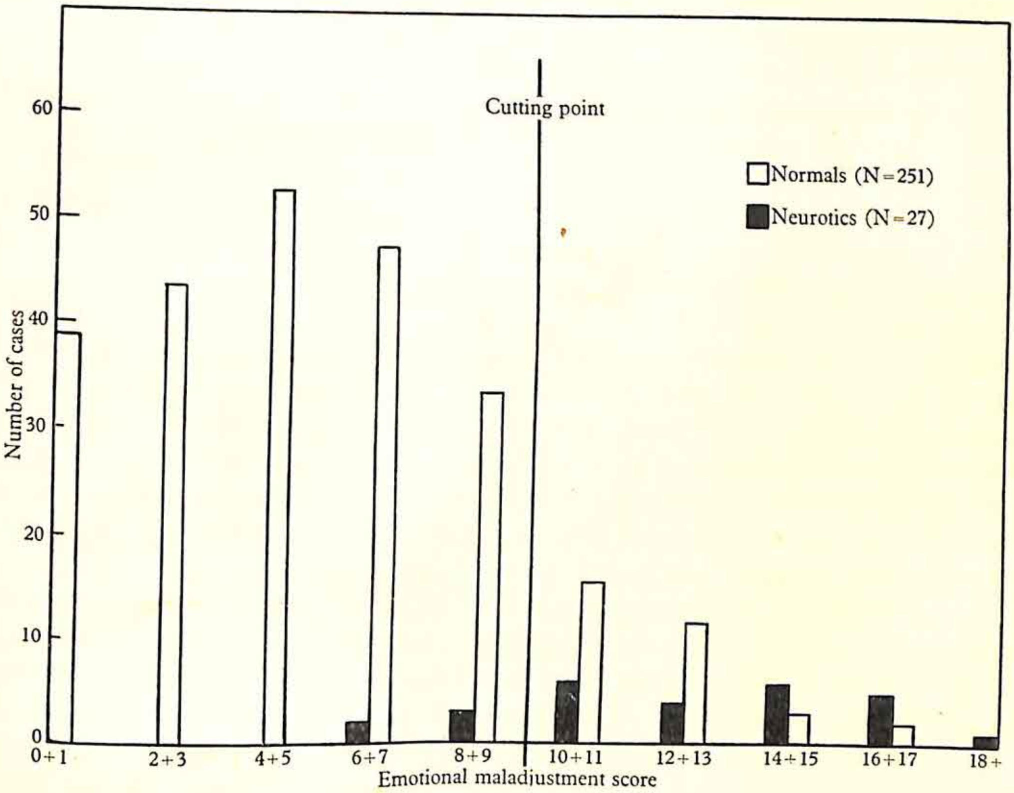

Fig. 1

For a fresh group ($N=100$) available as a check, the comparable figures are 13, 19 and 68%. In deciding to regard scores 0–7 inclusive as justifying a classification of 'probably well adjusted', the writer does not overlook the comments of Super (1942) and of Landis & Katz (1934) on the Bernreuter Inventory. These writers expressed doubts concerning the inferences to be drawn from a low score, with which the present investigator concurs. It is indeed unsafe to assume that a respondent obtaining a score lower than 8 on the present measure *is* well adjusted; all that can be said is that in the light of the available evidence such a respondent is likely to be well adjusted, and that the probability that this is so is reasonably high.

In view of data published elsewhere (Heron, 1956) it is essential to stress that these cutting-points and percentages cannot be considered to hold if the personality measure is used under conditions other than those usually obtaining in a research situation, and *certainly not under those obtaining in employment selection*. In brief, this is because the evidence referred to makes it clear that selection conditions result in a general downward shift of the score distribution.

The second part of the measure consists of thirty-six items, twelve of which are used as the basis for a score of sociability. Average response time for this section is about 8 min. The odd-even self-correlation of this twelve-item score, corrected to full length, is 0·74 ($N = 378$), which is surprisingly high unless one pauses to consider the probably beneficial consequences of rigorous item analysis. Each item discriminates significantly at the 5% level or better between lower, middle and upper thirds of total score, when use is made of χ^2 on observed differences from chance expectancy. In addition, ten of the twelve sociability items fail to achieve a significantly non-chance discrimination in terms of emotional maladjustment total score (all P's greater than 0·1), and the remaining two items cancel each other by obtaining P's of 0·05 in opposite directions. When the independent χ^2 for the twelve items are summated, account being taken of direction, the net additive χ^2 is 17·55, which for (12–1) (3–1) = 22 degrees of freedom gives $P > 0{\cdot}7$. From these data it may reasonably be concluded that in the selection of these twelve items to represent sociability while not also measuring emotional maladjustment, a considerable degree of success has been achieved. This conclusion has been sustained by factor analysis of the intercorrelations between the thirty-two items, using the average intercorrelations obtained from two stratified samples of 100 subjects each. At this point it is essential to be clear about the *a priori* assumptions one is using. The author would argue that the great weight of clinical and ordinary observational evidence supports an assumption that healthy individuals may differ in the degree of sociability characterizing their behaviour, quite independently of their emotional adjustment to the social and natural environment. The contrary, however, does not appear to be equally true. Those people admitted for treatment as 'emotionally maladjusted' (or 'neurotic') are far more often withdrawn and asocial than the reverse; in other terms, there are far more dysthymics than true hysterics to be found among the in-patient and out-patient populations of psychiatric hospitals. If then one essays to produce pure measures of sociability and of emotional maladjustment, it may prove more difficult in respect of the latter variable. With these considerations in mind one may now examine the present findings.

Three factors were extracted (the third solely to provide rotational freedom) and the two expected factors of 'emotional maladjustment' and 'sociability' were clearly identifiable after two straightforward orthogonal rotations. The amounts contributed to the total variance accounted for by each of the factors can be apportioned between the twelve 'sociability' and the twenty 'emotional maladjustment' items respectively. When a correction is made to equate for number of items (at sixteen, the mean of twelve and twenty) it is found that the 'sociability' *items* only contribute 11% of the variance accounted for by the 'emotional maladjustment' *factor*. Conversely, however, the 'emotional maladjustment' items contribute 21% of the variance accounted for by the 'sociability' *factor*. From this it is clear that overlap between the two factors is mainly due to the impurity of items chosen to measure 'emotional maladjustment'. If one feels justified in assuming *a priori* that emotional maladjustment does not, on our pattern of culture, involve any element of withdrawal behaviour such as might be termed 'unsociableness', then a clear course of action is open. One need only discard items which measure both factors until one is left with as pure a group of items for emotional maladjustment as one has already obtained for sociability. However, examination of the raw data concerning the twenty items used in the present study to represent emotional maladjustment raises the very problem anticipated in the theoretical discussion above. The items which one would discard in the interests of factorial purity tend to be those which most sharply discriminated between the validation groups of hospitalized neurotics and of apparently healthy people. One is therefore left wondering what a pure measure of 'emotional maladjustment' would represent when shorn of these items. By this time it might be safer to term the measure 'X'. However, it could reasonably be argued that for practical purposes this would not matter, since to locate the emotionally maladjusted in any population under study one would simply use the bivariate distribution provided by the two 'pure' scores, and regard those in the appropriate quadrant (low 'sociability' plus high 'X') as 'emotionally maladjusted'. Whether such a pragmatic approach would be theoretically satisfying would then remain a matter for debate and further research. The present instrument does not, however, lend itself to such treatment, which would, quite apart from theoretical considerations, require a very large pool of items. Use of such a large inventory would itself raise a fresh set of problems in the area of response motivation, to which reference has already been made.

The plan of development for the present two-part inventory required a valid, self-consistent measure of emotional maladjustment (whether this involved elements of pathological withdrawal behaviour or not) and a valid, self-consistent measure of *normal* sociability which could be used successfully as a criterion when no question of emotional maladjustment was involved. These objectives are considered to have been largely achieved. External evidence of validity for the sociability score must depend on acceptance of the earlier assumption relating 'introversion-extraversion' to 'anxiety-hysteria', but in this connexion some additional data have been obtained. An objective measure discriminating between patients diagnosed as 'dysthymic' and 'hysteric' respectively was evolved by Venables (1955). It consisted of a motor task carried out first under 'easy' and then under 'difficult' conditions. He found that hysterics showed little or no change in performance from 'easy' to 'difficult', whereas dysthymics tended to become more 'overactive' (Davis, 1948) under 'difficult' conditions. For twenty-four hospitalized neurotics the tetrachoric correlation was 0·64 ($P = 0{\cdot}001$) between Venables's measure and the twelve-item sociability score when a 13/11 split was made at the point of minimum misclassification.

Table 2 lists for information and interest the items used as a basis for the present total scores for emotional maladjustment and sociability, but it is strongly emphasized that no evidence is yet available which would justify extending the characteristics of these scores here reported to their use in isolation from the remaining buffer items. Psychologists wishing to use the two-part measure are invited to approach the writer for a copy of the full inventory and accompanying instructions.

Sources quoted are those from which the items were actually taken, but it is realized that many items also appear in slightly different form in other earlier inventories.

VI. ALTERNATIVE METHODS OF ADMINISTRATION

In a paper read before a meeting of the British Psychological Society (Heron, 1953), the writer described a new method of administering inventories of this type. At the close of that paper, considerable emphasis was laid on the fact that no evidence was then available

Bureau Ednl. Psy. Research
DAVID HA [illegible] NING COLLEGE
Dated ....

Table 2. *List of items finally selected as basis for scoring*

First Part (emotional maladjustment)	S.R.	S.
3. I sweat very easily even on cool days	*T*	*w*
5. I am troubled by sick headaches	*T*	*u*
6. My feelings are easily hurt	*T*	*m*
7. Sometimes when I am not feeling well I am cross	*T*	*l*
9. I consider myself rather a nervous person	*T*	*m*
15. I never have nightmares	*F*	*m*
20. I wake up fresh and rested most mornings	*F*	*w*
23. My eyesight is sometimes blurred	*T*	*p*
25. I worry over possible misfortunes	*T*	*m*
27. It is difficult to irritate me	*F*	*m*
33. My bowels are not opened every day	*T*	*p*
44. My physique is excellent	*F*	*p*
46. Once in a while I think of things too bad to talk about	*T*	*l*
47. I often feel self-conscious in the presence of superiors	*T*	*m*
50. I feel that very little works out the way it should in life	*T*	*c*
55. I have no particular physical or health problems	*F*	*u*
60. I have periods of such great restlessness that I cannot sit long in a chair	*T*	*w*
61. My hands often feel damp and clammy	*T*	*u*
69. I catch cold frequently	*T*	*p*
72. I often jump to conclusions	*T*	*r*
Second Part (sociability)		
79. The presence of the opposite sex makes me feel shy	*T*	—
82. What others think of my actions does not bother me much	*F*	—
83. I sometimes treat my work as if it were a matter of life and death	*T*	—
84. I like to play practical jokes on people	*F*	—
86. I find it easy to act naturally at a party	*F*	—
93. I like to have time to be alone with my thoughts	*T*	—
97. I like to have a lot of social engagements	*F*	—
101. I tend to be overconscientious	*T*	—
102. I am usually a good mixer	*F*	—
103. I think of myself as a lively sort of person	*F*	—
104. When I am bored I feel like stirring up some excitement	*F*	—
109. At a gay party I can usually let myself go and have a thoroughly good time	*F*	—

S.R. significant response; S. source.

Key: *w*, Winne's MMPI study; *u*, U.S. Army; *m*, Maudsley Medical Questionnaire; *l*, Lie scale, MMPI; *p*, physical health buffer item originally; *c*, Cattell; *r*, rhathymia item transferred; *T*, response significant if marked 'True'; *F*, if marked 'Not true' (=False).

Note. The 'significant responses' shown in the second part are in the '*un*sociable' direction for convenience in scoring.

to show what difference if any might be made as a result of using the new method as compared with a conventional pencil-and-paper approach. It is now possible to state that so far as means of total score are concerned, no significant differences arise between the two methods. The new method of administration, which employs a sealed box and cards, has, however, been described by several users as generally more acceptable than the usual pencil-and-paper presentation, possibly because it provides a certain amount of response motivation.

Only those who have tried to gather large-scale data will appreciate how greatly the writer has depended on the help of others. His first expression of appreciation, none the less real because perforce anonymous, must go to the 1696 men and women who have during the past four years completed responses to this inventory, at various stages of its development, and by one or other of the methods of administration.

Prof. H. J. Eysenck assisted with advice and facilities when both were especially needed. Mr A. E. Maxwell, M.A., frequently gave valuable statistical advice during the final stage, including the specific suggestion which led to the first-stage item analysis procedure.

Prof. Rex Knight, Miss Betty Fraser, Mrs Lorna Simpson, and students at the University of Aberdeen combined forces to ensure that responses from university students, a category usually responsible for all the data concerning measures such as these, were not on this occasion wholly excluded. To Ian Haldane, M.A., of the Army Operational Research Group, and Inspector F. R. Priestley of the Essex County Police goes appreciation for their cooperation in extending the range of the main sample.

The co-operation of workers in electrical engineering factories was obtained through the courtesy of management and trade union at Ferranti Ltd; of new entrants as omnibus conductors in Manchester, by the help of the General Manager, A. F. Neal, B.Sc. (Eng.), Mr J. F. J. Webb, Personnel Manager, Mr E. Darby, Chief Instructor of the Training School, and Councillor Thomas, Transport and General Workers Union. Similar assistance in London was obtained through the good offices of the Chief Medical Officer, Dr L. G. Norman, and of the Recruiting, Training and Education Officer, Mr K. R. Thomas, and their staffs at the Central Recruiting Office.

The development of this criterion formed part of a long-term programme of research into the association between psychological and occupational handicap, under the general direction of Prof. Aubrey Lewis, M.D., F.R.C.P., whose comments and criticisms have been sincerely appreciated. The criticism and assistance of Unit colleagues is also gratefully acknowledged.

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SOME EXPERIMENTS ON PAVLOV'S THEORY OF SUGGESTION*

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Pavlov's theory implies that some degree of inhibition should be present following a period of suggestion and that the amount of inhibition will to some extent determine the response to suggestion. Experiments were carried out to determine whether changes in auditory sensitivity following suggestion were in accordance with this theory. As predicted, suggestions of arm movement were followed by increased thresholds. A period of prose reading of the same duration and intensity produced the same effect but casual conversation did not. Correlations between suggestibility and threshold increment were, on the whole, positive but not large enough to justify a definite conclusion. The threshold increment was greater in subjects who had been taking sedative doses of sodium amytal for one week.

I. Introduction

A response to suggestion has often been said to be determined by two processes, usually antagonistic. Eysenck (1947), for instance, spoke of 'aptitude' or ideo-motor tendency and 'attitude' or the conscious inhibition of this tendency. Hull (1933) put forward a similar proposal. Pavlov (1927) explained suggestion in terms of his general theory of brain function. This led him to suggest one way in which the two processes might interact. According to him, suggestion causes a concentration of excitation in part of the cortex while other areas are inhibited by negative induction. This diminishes the effect of influences which would otherwise tend to counteract the suggestion.

From Pavlov's theory one can predict first that some degree of inhibition will be present following a period of suggestion, and secondly that the amount of inhibition will to some extent determine the response to suggestion. There should in fact be a positive correlation between response and inhibition. The object of the first experiment was to test both these predictions.

II. Experiment 1

(i) *Method*

Monaural auditory thresholds were determined before and after a test of suggestibility. The horizontal arm-movement test (Ingham, 1955) was used, the suggestions lasting for 2·5 min. Changes in threshold were compared with those in a control group of different individuals for whom, instead of suggestion, a period of casual conversation intervened between the threshold measurements. Subjects wore headphones, and the stimulus for the hearing tests was a 400 cycles per sec. tone presented to the left ear. Thresholds were determined by the method of limits using series of increasing intensities. Three such determinations were made before the suggestion or control period and three after. Before this, however, six or more preliminary practice determinations were made. All these measurements were made in groups of three with a rest interval 3–4 min. between groups.

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* This article is based on a paper read at the Annual Conference of the British Psychological Society in Durham, April 1955.

Within each group there was half a minute between the completion of one determination and the beginning of the next. The subliminal intensity of the tone at the start of the threshold determinations was varied throughout the practice groups but was otherwise held constant. Catch tests were introduced during the practice groups.

Suggestions were given by means of a tape recorder connected to the right headphone, the gain control being set to give comfortably audible speech reproduction. During the equivalent intervening time interval the control subjects kept their headphones in position but received no sound stimulus through them. They conversed with the experimenter who avoided topics likely to evoke emotional reactions.

The subjects were male adult neurotic in-patients selected as being co-operative and sufficiently intelligent to follow the instructions. Patients with a previously detected impairment of hearing were excluded. Seven subjects were allotted at random to each group and the experiment was repeated on five successive days for each subject.

(ii) *Results*

In Figs. 1 and 2 are the mean thresholds before and after the suggestion or control period. These are the results for the first 2 days only. All 5 days showed a similar pattern and, after Exp. 2, only two repetitions per subject were used.

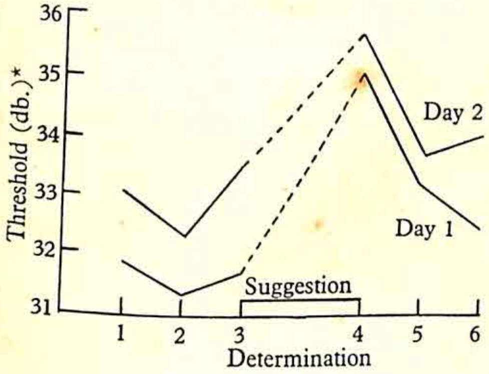

Fig. 1. Exp. 1 mean thresholds (experimental group).
* Reference level = 0·001 mV. (r.m.s.) at headphone.

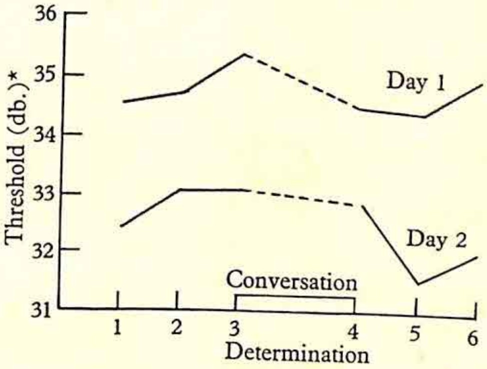

Fig. 2. Exp. 1 mean thresholds (control group).
* See footnote to Fig. 1.

For each session and for each subject the arithmetic mean of determinations 1, 2 and 3 was subtracted from determination 4. This difference will be called the threshold increment. The mean threshold increments for the 2 days were +3·1 db. (experimental group) and −0·2 db. (control group). There was very little overlap between the two groups, and the probability of getting as large a difference in the predicted direction by chance is less than 0·005 ($t = 3{\cdot}34$). There was no overlap between the groups when the mean threshold increments for the 5 days were compared. It appears, therefore, that the first prediction has been verified; there is an increase in auditory threshold following a period of suggestion.

Table 1 shows the rank correlations (τ) (Kendall, 1948) within the experimental group between suggestibility and threshold increment. They are consistent with the second prediction, that there should be a positive correlation between the two variates, but the levels of significance do not justify a definite conclusion.

Table 1. *Correlation between suggestibility and threshold increment* ($N=7$)

Day	τ	P
1	+0·52	0·068
2	+0·39	(0·19)*
3	+0·35	0·19
4	−0·16	—
5	+0·39	0·15
Total	+0·33	0·19

* Approximately.

III. Experiment 2

(i) *Method*

The object was to repeat Exp. 1 using different control conditions. This time the control subjects listened to an auditory stimulus of the same duration and intensity as the suggestion. A descriptive passage from Hardy's novel *The Woodlanders* was used. In order to equate the intensities as far as possible it was necessary to make a new recording of the suggestions. There were other slight changes in technique aimed at reducing experimental errors. Subjects were selected from a similar group to those in Exp. 1. Ten were allotted at random to the experimental group and the remaining eleven to the control group.

(ii) *Results*

Average thresholds are shown in Figs. 3 and 4. Although the first post-suggestion average threshold was slightly higher than the three initial points on both graphs for experimental subjects, it was clear that the marked increments obtained previously were no longer occurring. There was no significant difference in threshold increment between experimental and control groups. There was again some slight evidence of a correlation between threshold increment and suggestibility (Table 2).

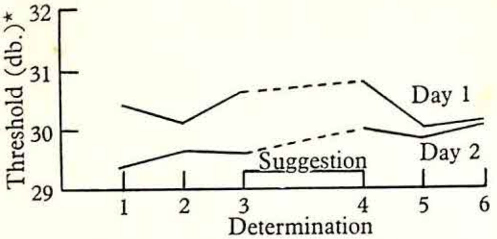

Fig. 3. Exp. 2 mean thresholds (experimental group). * See footnote to Fig. 1.

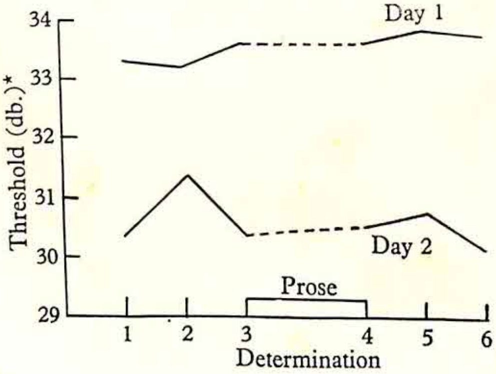

Fig. 4. Exp. 2 mean thresholds (control group). * See footnote to Fig. 1.

There was clearly a discrepancy between these results and those of the first experiment. The probability of both sets of threshold increments following suggestion being selected by chance from the same population was one in four hundred. It became necessary to ascertain the reason for this discrepancy.

IV. EXPERIMENT 3

The procedures used in the first two experiments were compared for differences which might account for the failure to produce the effect the second time. Two changes which had been made in Exp. 2 were the introduction of a new recording of the suggestions and the elimination of a click in the headphone which had previously followed the suggestion. To see whether either the old recording or the click were responsible for the effect in Exp. 1 a group of similar patients were tested once using the old recording without the click and once using the new recording with the click. Subjects were tested on successive days in counterbalanced order. In neither case was the result materially different from that of Exp. 2, and there was no significant difference between mean increments under the two conditions. Correlations between threshold increment and suggestibility ($\tau = -0{\cdot}1$ and $0{\cdot}0$) were insignificant.

Table 2. *Correlation between suggestibility and threshold increment* ($N = 10$)

Day	τ	P
1	+0·25	0·19
2	−0·51	—
3	+0·11	0·36
4	+0·64	0·0046
5	+0·33	0·108
Total	+0·33	0·108

V. EXPERIMENT 4

(i) *Method*

Another possibility emerged from an examination of the medical records of the subjects. It was found that most of the patients taking part in the first experiment were, and had been for some time, receiving fairly large doses of sedatives, sodium amytal in most cases. The subjects of the later experiments, however, were rarely sedated, and the few who were, had received considerably smaller doses. The object of Exp. 4 was to see whether this could account for the difference between the two sets of results. Exp. 2 was repeated using two groups of patients, one receiving sodium amytal in doses comparable with those of Exp. 1 and the other unsedated. The subjects, all neurotic patients, were allotted at random to the sedated or unsedated groups. Six of the sedated subjects received 9 grains per day given orally and the other nine received 4 grains per day by the same route. The medication had continued for 1 week prior to the experiment and the unsedated subjects had remained unsedated during the same period. Two experimental sessions per subject were undertaken on successive days, one under the experimental conditions (i.e. with suggestion intervening between the two sets of threshold measurements) and one under the control conditions (i.e. with a passage of prose intervening). These were the stimulus conditions used in Exp. 2. The order of the experimental and control conditions was alternated at random throughout the groups. Two hypotheses to be tested were (1) that suggestion would produce a greater threshold increment than the control conditions and (2) that the sedated group would show greater threshold increments than the unsedated group. There were fifteen sedated subjects and thirteen unsedated.

(ii) *Results*

The average thresholds for the control and suggestion conditions are shown in Fig. 5. There was no significant difference between the two sedated groups which have therefore been combined. One point that seems immediately apparent is that these results do *not* support the first hypothesis. There was no significant difference between suggestion and control conditions in either sedated or unsedated groups.

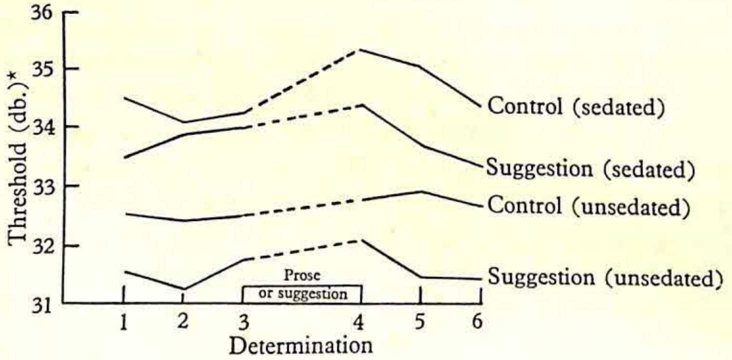

Fig. 5. Exp. 4 mean thresholds. * See footnote to Fig. 1.

Table 3. *Analysis of threshold increments in decibels (mean of* 2 *days)*

	N	Mean	t	P
Exp. 4: Sedated	15	+0·96	3·22	<0·005
Unsedated	13	+0·46	2·24	<0·025
Exp. 1: Sedated (controls)	6	−0·06	(1·77)*	(<0·05)*

* Refers to the difference between Exp. 1 sedated (controls) and Exp. 4 sedated.

Table 3, however, shows that there is a significant increase in threshold following the period of stimulation for both sedated and unsedated groups. The lower row shows that the mean increment for the sedated controls in Exp. 1 (with casual conversation during the control period) was slightly less than zero and was significantly less than that for the sedated group of Exp. 4.

Pavlov's theory specifies that suggestion should produce inhibition by negative induction. A difficulty arises, however, when we try to specify stimulus conditions under which the same phenomenon would *not* be expected to occur. According to Pavlov a stimulus exciting the cortex may cause one of two effects. Either the excitation is concentrated, in which case it induces inhibition in surrounding areas, decreasing the excitability of these areas, or, alternatively, the excitation irradiates, thereby increasing the excitability of surrounding areas. Both irradiation and concentration may occur at different stages during and following the action of a single stimulus. Unfortunately, he was unable to specify unambiguously the conditions under which one or the other would predominate. Because of this it is difficult to set up, with any conviction, a stimulus situation which would *not* be expected to produce negative induction.

When a passage of prose was chosen for the control situation, the implicit hypothesis was that the repetitive quality of the suggestion was the characteristic necessary to produce negative induction. The effect of such a stimulus was therefore compared with another which was not repetitive but included a variety of ideas in a passage of descriptive writing. The results are not in accord with the hypothesis. An important problem for future research is to discover the feature or features responsible for producing the inhibitory effect in those situations where it occurs.

Let us now consider the second hypothesis, that the sedated group will show greater increments than the unsedated group. As shown in Table 3 the mean increment of the sedated group was 0·5 db. greater than that of the unsedated group. The probability of getting, by chance, as large a difference in this direction is between 0·10 and 0·05 using a t-test ($t = 1{\cdot}32$). Using a non-parametric method (Kendall, 1948) the probability is 0·06. As the difference was of borderline significance it was considered advisable to increase the size of the groups. It was possible to do this for the unsedated group by adding to it those subjects from the earlier experiments who had been tested under the same stimulus conditions (either suggestion or prose) and who were not receiving sedatives. The difference between the groups was now significant at the 5% level. The data, therefore, lend definite support to the hypothesis. It seems likely that the inhibitory effect is increased by sodium amytal under these conditions.

Does this account for the discrepancy between the results of Exp. 1, where the subjects were sedated, and those of Exps. 2 and 3 where they were not? In Exp. 1 six of the seven patients in the experimental group were taking sodium amytal at the time of the experiment. The mean threshold increment for these six was +3·55 db. The difference between this and the +0·96 db. which was the equivalent figure for the sedated subjects in Exp. 4 was significant ($t = 3{\cdot}50$ and $P < 0{\cdot}01$ using a two-tail test). It is clear that some factor is required to explain the results of the first experiment in addition to sedation. One such factor is the longer period of medication involved. The six sedated patients in Exp. 1 had been receiving sodium amytal as a part of their usual treatment for considerably longer than 1 week. Another possibility is that a modification of instructions made after Exp. 1 was sufficient to reduce the effect. Following the suggestion in Exp. 1 and preceding the first post-suggestion threshold measurement the investigator said 'listen again now please'. In later experiments the words '. . . tell me as soon as you can hear the sound' were added. It is possible that attention was more effectively concentrated on the tone by the second instruction. There may be other differences between the circumstances of the two experiments of which we are not aware.

In Exp. 4 there was no evidence of a correlation between suggestibility and threshold increment. A new apparatus was being used to measure arm movements, and an unanticipated source of error was discovered in the course of the investigation. It was felt that less reliance could be placed upon these suggestibility measurements than in Exps. 1 and 2, where some evidence of a positive correlation was found.

VI. CONCLUSIONS

The object of these experiments was to test two predictions from Pavlov's theory of suggestion. The main findings were:

(1) As predicted by the theory, a period of suggestion, given under the conditions of the arm-movement test, was followed by a reduction in auditory sensitivity.

(2) A period of prose reading, of the same duration and intensity and under the same conditions, produced the same effect.

(3) A period of casual conversation of the same duration produced no such effect.

(4) The second prediction (of a positive correlation between suggestibility and threshold increment) was neither confirmed nor disproved, but the evidence, as far as it goes, is slightly more for it than against it.

(5) The threshold increment was greater in subjects who had been taking sedative doses of sodium amytal for 1 week.

I am indebted to Mr M. B. Shapiro for the discussions which led to the formulation of the first experiment. Drs T. J. Hennelly, J. M. White, D. Lewis and C. J. Morgan kindly allowed me to use patients under their supervision as subjects, and Dr Morgan supervised the administration of drugs in Exp. 4. I also wish to thank the patients themselves for their willing co-operation. I am grateful to Dr Derek Richter, Director of Research, for his interest and advice.

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THE EFFECTS OF PROPAGANDA ON RECALL, RECOGNITION AND BEHAVIOUR

II. THE CONDITIONS WHICH DETERMINE THE RESPONSE TO PROPAGANDA

By EUNICE BELBIN

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A series of experiments were conducted in which Road Safety Posters were displayed in a waiting room. Their effect was measured by: (*a*) the amount of information from the posters which was applied to the interpretation of photographs showing faulty traffic situations (*Use* scores); and (*b*) the amount of information contained on the posters which could be recalled (*Recall* scores). An attempt was made to investigate the reasons why on some occasions or with some subjects the information was recalled more than it was used, and why on other occasions or by other subjects it was used more than it was recalled.

Four types of propaganda were displayed. Each had a significant effect on the use scores, but there was no significant difference between the effects of these four types on either use or recall. However, the previous experience of the subjects was shown to have a considerable effect on the results. The over-sixties, although comparatively bad at recalling the propaganda, had use scores which compared favourably with those of the younger subjects. The subjects who had car-driving experience used the information from the posters to a significantly greater extent than the non-drivers. On the other hand, the drivers did not recall the propaganda any better than the non-drivers. There was also a tendency for the naval rating subjects to use more of the information than they recalled and for the students to recall as much or more than they used. When the tests were applied up to a period of 14 days after the display of propaganda there was a widening disparity between use and recall scores. At 14 days subjects still showed a slight tendency to use the information although they were not able to remember it.

An attempt is made to explain these results by suggesting that when a subject is presented with an incidental learning situation, the meaningfulness he extracts from the material and his method of organizing it depends on his previous learned responses to similar material. Where his previous experience has entailed incorporation of the information into his activity, then the new material to be learned is likely to have meaning in relation to its practical application, a response which favours the retention of the information for use in other tasks, but not necessarily for recall. If his previous experience has been learning by memorizing, his response to the new material will be organized accordingly; he is likely to learn in order to recall.

I. Introduction

In some previous experiments which have been conducted by the writer (Belbin, 1956) it became evident that the information which had been learned from propaganda material could become apparent in two distinct forms, namely, (*a*) in recall or recognition, and (*b*) in subsequent behaviour.* For example, when Road Safety Propaganda was presented to children in their Cinema Clubs it was found that although the propaganda had no measurable effect on their behaviour, they could nevertheless describe and recognize it quite well. Children who were best able to describe the propaganda material were not always those who behaved correctly on the roads. Again, when propaganda was used in relation to performance on a laboratory task it was found that some subjects were unable to describe it although it affected their behaviour. Others who could describe or recognize it were not influenced by it.

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* Throughout this paper the term behaviour has been used to denote non-verbal action.

These results are in conflict with the assumption which has hitherto been made that propaganda is likely to affect behaviour in proportion to the extent to which it can be recognized or recalled. It was not clear, however, from these experiments what caused the different effects to be more marked in some subjects than in others. The following series of experiments (reported more fully elsewhere, see Belbin, 1953) were therefore designed to examine this problem. In these experiments we have tried to discover the reasons why the information contained in propaganda should on some occasions be remembered but not used and at other times be used but not remembered. We have examined the extent to which these different responses might be a function of: (1) the type of propaganda; (2) the characteristics or experience of the subject; and (3) the length of time between display of the propaganda and the measure of its effect.

II. EXPERIMENTAL PROCEDURE

Six Road Safety Propaganda Posters were displayed in a waiting room. Subjects were left in this room without instruction for 3 min. Afterwards they were asked to point out, in some photographs of local traffic scenes, any faults they could find in relation to Road Safety. Each of six photographs (test photos) was directly related to one of the six posters, the purpose of the experiment being to find the effect of the posters on the interpretation of the photographs. A further eighteen photographs (non-test photos) which were not related to the posters were shown to the subjects along with the test photos. After the photograph test, the subjects were asked to describe the posters which they had seen in the waiting room. A control group of subjects was asked to interpret the same set of photographs without propaganda having been displayed previously. The experimental design was therefore as follows:

Experimental group	Control group
1. Display of posters (3 min.)	—
2. Photo test (18 non-test photos, 6 test photos)	Photo test (18 non-test photos, 6 test photos)
3. Recall test (description of photos)	—

It was thus possible to compare under various conditions: (*a*) the extent to which the information contained in the posters was applied to the photo test (use scores); and (*b*) the extent to which the propaganda was remembered (recall scores). Subjects scored one mark on use scores for each test photo when the reply was in accordance with the propaganda. For recall scores one mark was scored when it was clear from the description that the poster had been correctly interpreted.

Four different types of propaganda were shown: (*a*) pictorial; (*b*) horror; (*c*) verbal positive; and (*d*) verbal negative. There were six posters for each type, the slogans of these six for types (*a*), (*b*) and (*c*) being as follows: (1) Dog-owners! Keep your dog on a lead. (2) Cyclists! Keep well away from stationary cars—the door may suddenly open. (3) Pedestrians! Give way to traffic on crossings when the policeman signals it on. (4) Cyclists! Only very small children may be carried as passengers. (5) Pedestrians! Always face the oncoming traffic. (6) Cyclists! Strap your parcels on. A hanging package may catch in the wheel.

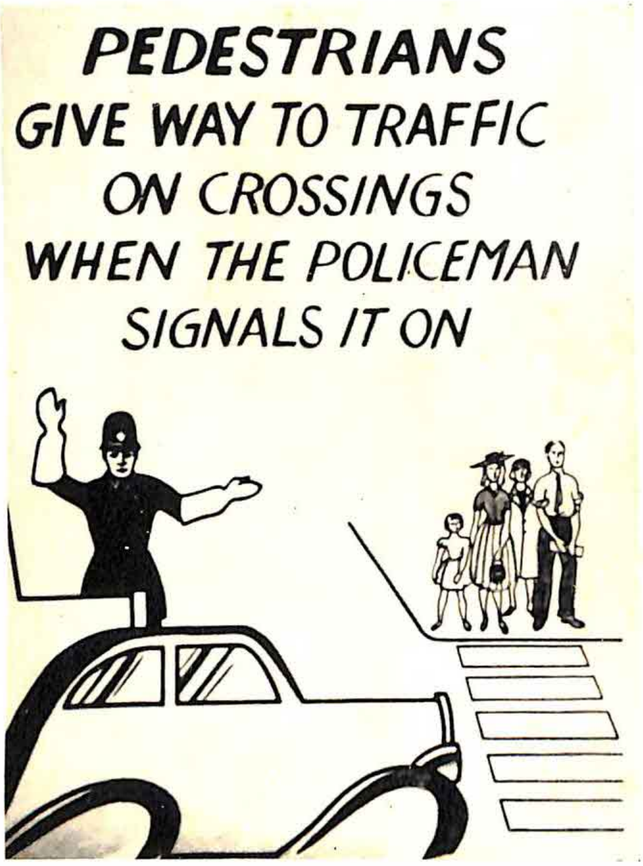


Fig. 1. Copy of one of the posters which was used in the experiment. (Pictorial Propaganda Number 3)

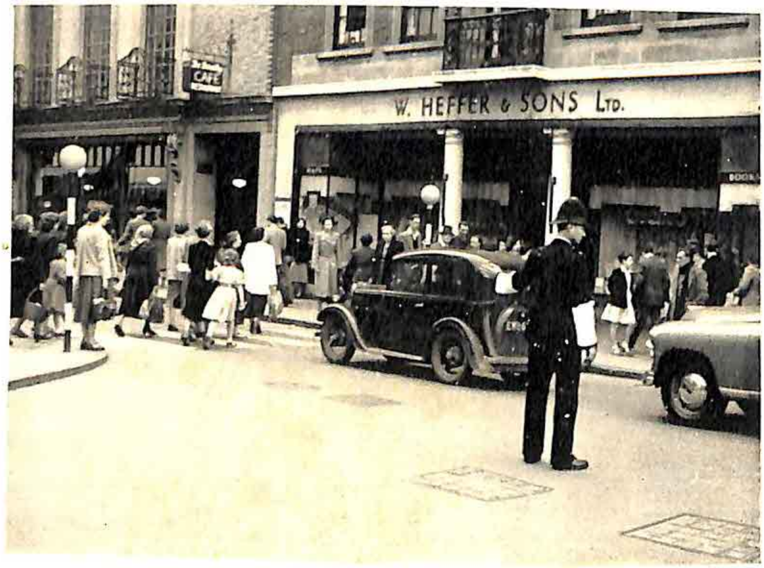


Fig. 2. The Test Photo which was related to the Poster shown in fig. 1.

Type (*c*) merely carried these slogans; type (*a*) also showed pictures of the correct behaviour to be used; type (*b*) displayed accidents which were resultant upon incorrect behaviour; and type (*d*) contained the same messages but in negative form, e.g. no. 5 was 'Pedestrians! Never walk with your back to the traffic.' Pl. 1, fig. 1, shows one of the pictorial posters used and Pl. 1, fig. 2 shows the corresponding photograph which was included in the subsequent test.

The comparative effects of these four different types of propaganda were observed both in use and recall scores; (*a*) with a group of fifty-seven naval ratings aged 18–22 years; and (*b*) with a group of fifty-five university students aged 18–22 years. The effect of the pictorial propaganda was also assessed on: (*c*) a group of men and women aged 65–75 years who were drawn from the Over-Sixties Clubs ($N=21$). In all three cases part of the group acted as a Control and did not see the propaganda, but merely interpreted the photographs. Finally, the test was presented with different time intervals between the display of the propaganda and the photo test to: (*d*) six groups each comprising five men and five women aged 18–22 years. The men were university students; the women were from a Teachers' Training College.

III. RESULTS

(1) *The effect of type of propaganda*

The mean scores for the groups of students and naval ratings after each of the four types of propaganda are shown in Table 1, together with the values of $S\sqrt{(\text{var }S)}$ (rank correlation, Whitfield's method (1947)) when the use scores of each group are compared with those of the control group. It may be noted that: (*a*) All types of propaganda significantly affected the use scores on the photo test. This was true for both students and for naval ratings. (*b*) The propaganda had no significantly greater or less effect on the use scores of the students than on those of the naval ratings. (*c*) The greatest difference in use scores within the groups was between pictorial and horror propaganda for the naval ratings, pictorial being the more effective. The difference was not significant ($S\sqrt{(\text{var }S)}=0{\cdot}75$). (*d*) There was no significant difference between the amounts recalled after different types of propaganda. And (*e*) when the differences between use and recall scores (use minus recall) were considered for each type of propaganda, no significant difference was found between any two types.

(2) *The characteristics or experience of the subject*

Although there was no significant difference between the mean scores of the naval ratings and of the students after the different types of propaganda, there was, nevertheless, considerable individual variation within the groups. Information concerning the naval ratings which had been recorded during the experiment was analysed to see whether it could account for any of these differences.

(*a*) *Car driving.* The scores of the naval-rating drivers (or motor cyclists) were compared with those of the naval rating non-drivers. For every type of propaganda the drivers had higher mean use scores than the non-drivers. For example, after Pictorial Propaganda the mean use score of drivers ($N=6$) was 5·0 as compared with 2·6 for non-drivers ($N=8$). There was a significant difference between the use scores of drivers ($N=15$) and those of non-drivers ($N=30$) when all the scores for the different types of propaganda were taken

Table 1. *Use and recall scores after different types of propaganda*

	Use				
	No. in group	Mean score (6 photos)	S^* $\sqrt{(\text{var } S)}$	Sig. at* 5% level (i.e. $>1{\cdot}96$)	Recall Mean score (6 posters)
Naval ratings:					
Control group	12	1·4	—	—	—
Pictorial	14	3·6	3·2	Yes	2·6
Horror	10	2·9	2·4	Yes	2·6
Verbal positive	11	3·1	2·9	Yes	1·9
Verbal negative	10	3·2	3·2	Yes	2·9
Students:					
Control group	12	1·8	—	—	—
Pictorial	11	3·6	2·9	Yes	3·1
Horror	11	3·6	2·4	Yes	2·2
Verbal positive	10	3·5	2·5	Yes	2·8
Verbal negative	11	3·0	2·1	Yes	1·9

* When the scores after each type of propaganda are compared with the Control group scores.

together. Yet no propaganda was specifically addressed to drivers; two posters seemed quite inappropriate to them. There was no evidence to suggest that these results were determined or biased by any particular poster or posters.

This difference in the effect of propaganda on drivers as compared with non-drivers *cannot* be explained by the fact that car drivers were better at interpreting road safety photographs in any case. The mean scores on the eighteen non-test photos were as follows: drivers ($N=10$) 15·1; and non-drivers ($N=10$) 15·3. Thus without propaganda naval-rating drivers were slightly worse than non-drivers; with propaganda, they were significantly better. Nor can it be explained by the fact that drivers were more 'intelligent' than non-drivers, for there was no significant relationship between I.Q. and car-driving. Further, this difference cannot be due to the fact that car drivers had read the Highway Code. If this had accounted for the difference in performance, one would expect the scores on all photos to be higher for car drivers than for non-drivers. (This was not so, as shown above.) Secondly, two of the posters were intentional deviations from the Code, as follows:

Highway code instructions	Poster slogans
"...do not carry a passenger on your cycle unless it is constructed or adapted for the purpose	Only very small children may be carried as passengers
"...Motorists—before opening any door of a vehicle make sure that...and that you will not inconvenience anyone on the road or footpath...'	Cyclists—keep well away from stationary cars. The door may suddenly open

It might have been expected that, if the Highway Code directly influenced the results, the drivers' responses to the photos would have been in accordance with the instructions in it rather than in accordance with the poster instructions. In actual fact 87% of the drivers' responses were in accordance with the 'carrying passengers' poster (compared with 50% of those of the non-drivers) and 67% of the drivers' responses were in accordance with the 'opening car doors' poster (compared with 47% of the non-drivers).

The numbers in the driver groups were too small to enable a comparison to be made between the relative effects of different types of propaganda on drivers and non-drivers. The numbers in the non-driving groups, however, were slightly larger. From a consideration of these scores type of propaganda seemed to be unimportant. It was not possible to make a similar analysis for the women students as too few of them drove cars.

Let us now consider the recall scores. A very different relationship between the response of drivers and of non-drivers is apparent. For example, after Pictorial Propaganda, whereas the use scores had been 5·0 for drivers ($N=6$) and 2·6 for non-drivers ($N=8$), the mean recall scores for these subjects were 3·0 and 2·4 respectively. Thus while there was a significant difference between the use scores for the two groups, no significant difference existed between their recall scores $S\sqrt{(\mathrm{var}S)}=0{\cdot}22$. For five drivers with recall scores of 3 or 4 (3, 3, 3, 3, 4) the use scores were 6, 6, 6, 6, 4. On the other hand, for six non-drivers with recall scores of 3 or 4 (3, 3, 3, 3, 3, 4), the use scores were 3, 3, 3, 1, 4, 2. Similar effects could be noted after the display of Horror Propaganda.

It was not possible to consider whether cycling experience had similar effects to those of car driving. Nearly every subject was a cyclist.

(*b*) *Accidents.* It was thought that subjects who had had (or seen) bad accidents might have been more responsive to the propaganda or might have remembered it better. However, there was a slight tendency for those who had not had accidents to score more both on recall and on use than those who had had accidents. Use minus recall differed very little for the two groups.

(*c*) *Intelligence quotient.* All the naval-rating subjects were given an A.H. 4 test. There was a significant positive correlation (at 5% level) after Pictorial Propaganda between A.H. 4 scores and use scores and between A.H. 4 scores and recall scores, but not after other types of propaganda. There was no positive relationship between 'intelligence' and general ability to interpret the photos. When the A.H. 4 scores were measured against non-test scores $r=-0{\cdot}05$. With the relatively homogeneous group of subjects, whose A.H. 4 scores fell mainly in the B category of the industrial norms, any relationship that might have existed between intelligence quotient and scores after the propaganda might easily have been masked. For example, eight out of ten subjects in the Horror group were in category B of the industrial norms. There was, however, a negative correlation ($r=-0{\cdot}58$) between A.H. 4 score and number of misinterpretations of the meaning of the Horror Propaganda (apparent from the descriptions in the recall test), i.e. the less 'intelligent' were more likely to misinterpret the posters. Similarly, naval ratings misinterpreted the Horror posters one and a half times as often as the students. (There were only a few misinterpretations with the other types of propaganda.)

(*d*) *Sex differences.* It was not possible to consider sex differences with the foregoing data. For one reason most of the women students were non-drivers. However, a group of men students (aged 18–22) were tested after seeing the Pictorial Propaganda. They had a mean use score of 3·3 (when 10/11 were drivers). The woman students had a mean use score of 3·6 (when 10/11 were non-drivers). It is possible, therefore, that the women students might have been more influenced by the Pictorial Propaganda than the men students if the groups could have been equated in respect of driving experience. The mean recall scores for the two groups were: women, 3·1; men, 3·2.

(*e*) *Age differences.* Since there is a marked increase in the national pedestrian accident rate of people over the age of 65, the effect of propaganda on this group is of great practical importance. As most of the subjects, drawn from the Over-Sixties Club, were over 70, they were unlikely to have had recent experience in riding bicycles or driving cars, and it was therefore thought reasonable to consider only the effect of the three posters most relevant to the group (i.e. posters 1, 3 and 5). In addition, a number of the subjects were short-sighted. A smaller waiting room was therefore used where they were able to sit within a few feet of the display posters. When their mean scores were doubled (i.e. to enable a rough comparison to be made with the groups who had seen all six posters) the Control group ($N=13$) had a mean use score of 1·54 as compared with 3·26 for the experimental group ($N=8$). $S\sqrt{(\text{var } S)}=2{\cdot}2$, i.e. significant at 5% level. Their mean recall score × 2 was 1·76. By comparing these figures with those in Table 1, it can be seen that although the older subjects were relatively worse at recalling the propaganda, their mean use score was relatively high. Although they did not remember the posters there is ample evidence that they had learned the information contained in them.

(3) *The effect of time on the use and recall of propaganda—the length of time between display of the propaganda and measurement of its effectiveness*

Hitherto, the photograph test had been presented immediately after the display of propaganda. This test was now given at intervals of from 1 to 14 days after the display of Pictorial Propaganda. Subjects visited the laboratory twice. On the first occasion they saw the Pictorial Propaganda in the waiting room and were asked to interpret a few non-test photos, and on the second occasion they were given the usual photograph test and were asked to describe the posters that they had seen on their previous visit. Sixty different student subjects were tested (thirty men and thirty women).

The mean use and recall scores of these subjects have been broken down into drivers and non-drivers and are shown in Table 2. It will be seen that there was again a contrast between the use scores of drivers and non-drivers. (On the test photos there was no significant difference between the mean scores of drivers/non-drivers in a control group of students who had not been subject to propaganda.) The drivers applied the information from the propaganda more than the non-drivers. On the other hand, the non-drivers tended to recall slightly more than the drivers, although there was no very marked or uniform difference between the recall scores of the two groups. It was decided that in any further comparisons the scores should be weighted for driving/non-driving (on the assumption that each group would contain 50% drivers and 50% non-drivers.) The weighted means are also shown in Table 2, from which it can be seen that there is a steady decline with time in the performance of the subjects both on the use and recall scores. The small rise in recall scores after 7 days can be accounted for entirely by the scores of the five women in the group, who, it is suspected, had discussed the posters. The mean score of the men after 7 days was 0·34. It is interesting to note, however, that whatever the reason for this rise, the use scores did not rise correspondingly.

When significance tests were applied to the data (unweighted) it was found that the propaganda no longer had any significant effect after 1 day. It can be seen from the table that while there was still some use of the information 2 weeks after the propaganda display, there was practically no ability to remember the posters. The relationship between use and recall scores can be seen most clearly from a consideration of the effects of individual posters. For example, while subjects were not able to remember poster no. 4 at all after 2 weeks, the group was still better than the control group after this period at the photo test. There was a widening disparity with time between recall and use (Text-fig. 1).

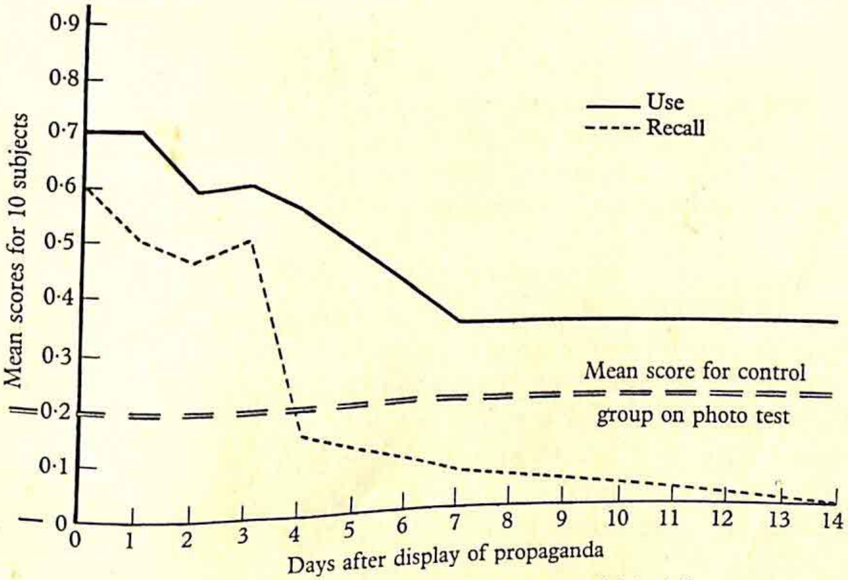
Let us now consider two general points in relation to all the preceding data. First, how much of the propaganda was recalled without being used? And secondly, how much of the propaganda was used but not recalled during the test?

Table 2. *The effect of time on the use and recall scores*

Students (five male and five female in each group)

Time interval between display of propaganda and test (days)	Mean use scores		Weighted mean use scores
	Drivers	Non-drivers	
1	4·25	2·0	3·13
2	3·0	2·6	2·8
3	2·6	2·5	2·55
4	2·7	2·3	2·5
7	2·0	2·1	2·05
14	2·3	2·0	2·15
	Mean score of Control group		1·8

Time interval between display of propaganda and test (days)	Mean recall scores		Weighted mean recall scores
	Drivers	Non-drivers	
1	2·0	1·4	1·7
2	1·0	1·4	1·2
3	0·6	1·2	0·9
4	0·3	0·6	0·45
7	0·7	1·1	0·9
14	0·3	0	0·15



Text-fig. 1. Mean use and recall scores (weighted for car driving) for one poster and photo (no. 4).

(4) *Propaganda recalled but not used*

An analysis of the responses of individual subjects shows that a number of posters were recalled although the relevant photos were not responded to accurately in the test. Table 3 shows the amount recalled but not used by both naval ratings and students.

Table 3. *Amounts recalled but not used by naval ratings and students*

	Percentage recalled of possible total responses	Percentage recalled but-not-used responses of possible total responses
Naval ratings:		
Pictorial	43	10
Horror	43	15
Verbal positive	32	5
Verbal negative	48	15
Students:		
Pictorial	52	11
Horror	37	3
Verbal positive	47	13
Verbal negative	32	8

(5) *Propaganda used but not recalled*

The number of posters which was used without being recalled is not subject to direct measurement. Although a number of photos was responded to accurately without the posters being recalled, these may well have been ones which subjects would have had correct in any case, i.e. without propaganda (cf. the control groups).

An attempt has been made, however, to determine very approximately how much of the propaganda was used without being recalled. In the photo test it was possible for a subject to make any of the following combinations of responses:

Photo correct + relevant poster recalled correctly = y responses,
Photo correct + relevant poster recalled incorrectly,
Photo correct + relevant poster not recalled = z responses,
Photo incorrect + relevant poster recalled correctly,
Photo incorrect + relevant poster recalled incorrectly,
Photo incorrect + relevant poster not recalled.

If the number of subjects in the group $= x$, and the number of photographs given to each subject $= a$, then the fraction $100\,\Sigma z/ax - \Sigma y$ can reasonably be compared with the percentage of responses in the control groups, i.e. we can ask the question 'other than those known to have been influenced by the posters, were there more in the experimental groups who Used the information than might have been expected from inspection of the control groups?' Table 4 shows a number of these comparisons. It can be seen that: (i) The value of $100\,\Sigma z/ax - \Sigma y$ for the student groups is remarkably consistent—whether it is for different types of propaganda or for the Pictorial Propaganda after different time intervals. (ii) The value of $100\,\Sigma z/ax - \Sigma y$ for the student groups differs very little from the percentage correct responses in the control group. (iii) In contrast to the student group, there was considerable variation between the naval rating groups. (The reason for the low score on the Horror group was that a number of posters were inaccurately recalled.) (iv) The over-sixties group, whilst having a control score comparable with that of the naval ratings and students, had a value for $100\,\Sigma z/ax - \Sigma y$ higher than for any other group, indicating that this group Used more posters without Recalling them than did any other group.

There is some evidence that with the student groups all types of propaganda were likely to be recalled as well as used. With the naval-rating groups there was a greater tendency to use information which could not be recalled, especially with Pictorial Propaganda. The over-sixties group showed the greatest tendency towards using information which could not be recalled.

Table 4. *Values of* $100\,\Sigma z/ax - \Sigma y$ *for different subjects and for different types of propaganda*

Type of propaganda	Naval ratings	Students	Over sixties
Pictorial (immediate)	40	30	44
Horror	12	32	—
Verbal positive	29	33	—
Verbal negative	30	34	—
Pictorial:			
1 day	—	31	—
2 days	—	28	—
3 days	—	30	—
4 days	—	36	—
7 days	—	32	—
14 days	—	32	—
Control group percentages of correct responses to the total possible responses	24	30	26

(6) *Attention to the propaganda and introspective evidence*

Finally, we must consider the introspection of the subjects to see whether any light is thrown on the problem of why some subjects used (or recalled) more information than they recalled (or used).

It was evident that even the most successful of the various types of propaganda was only used about half as much as it was possible to use it. One possible explanation is that the subjects did not look at the propaganda. Only two subjects of over 200 who were tested, however, said that they had not seen any posters. Their scores were omitted from the results. Proof that the posters were 'looked at' is that every subject described them as a homogeneous group. Many who could not recall the implication of the posters were able to say that 'two were for cyclists, one for pedestrians, etc'.

There is adequate evidence, however, that although all the subjects looked at the propaganda, some did not in fact perceive it. Many of the picture posters were described wrongly, e.g. 'Face oncoming traffic' was often interpreted as 'Don't cross in front of a bus'. Many subjects said that they looked at the posters merely from an artistic or critical point of view. They seemed to have perceived structural items, but not the total concept of the picture. Some had consciously rejected any idea of reading the slogans and thus of being able to understand the implication of the posters, e.g. 'I thought "Road Safety again" and didn't look any further.'

On the other hand, there were subjects who read and understood the implications of the posters, but who failed to change their attitude towards the particular problem, e.g. 'I read them all, but they didn't have the slightest effect on the Photo test, as I knew all the points already' (use score 1/6). There were still a number of subjects, however, who read, recalled and expressed appreciation of the propaganda and yet who failed to be influenced by it at all in the photo test, e.g. 'Propaganda does a lot of good—the more the better' (use score 1/6). It should be noted that none of the subjects was aware of the object of the experiment.

IV. Discussion

An analysis of the data in this experiment shows that, although the type of propaganda had little effect on the results, the relationship between recall and use of the propaganda varied in amount and direction both within and between different groups of subjects. We cannot account for these results by postulating that either recall or use was more sensitive as a measure of effectiveness. For although some subjects tended to use more propaganda than they could recall (e.g. the car drivers and over-sixties), others used most of the propaganda which they were able to recall (e.g. non-drivers). Also, there were some subjects who tended to Recall more than they used. One argument which might be put forward is that individual items of the propaganda were recalled, although the propaganda was not understood as a meaningful whole and, in consequence, did not affect behaviour. As Vernon (1951) states '...memorization of particular words and phrases, and even of whole statements, can be achieved without any assimilation of the general argument or understanding of the ultimate meaning of a written passage'. It is hardly likely, however, that the one sentence slogans used in this experiment could have been recalled without being understood. A second argument is that some of the propaganda might have been understood and recalled but not accepted as useful information. This is unlikely to have accounted for the performance of many subjects, as so many of them voluntarily expressed approval of the slogans which they had recalled but not used.

Welborn & English (1937) have summarized a considerable amount of experimental work which has been designed to show the difference or similarity between 'verbatim' and 'substance' learning. Most of the tests of substance learning, however, have either required a verbal or written précis of the original or have been direct question objective tests. The influence of the learning on subsequent behaviour has not been measured and the results cannot therefore be adequately compared with our own. Katona (1940) developed the distinction between learning by memorizing and learning substance one stage further in the field of problem solving. But in none of this previous work has verbal material been learned and then the information transferred to a non-verbal task. Szekely (1950), however, has examined the question of whether a functional connection exists between insightful previous learning and subsequent problem solving by studying the effects of two different ways of learning verbal material. While one group (the 'modern' method group) learned the material by incorporating it into their activity, the others ('traditional' method group) merely learned the substance. The learning material was the solution of a simple mechanical task. The solution of the final task involved the knowledge of the principles which had been learned in the first task. He found that the frequency of solution in the final test was considerably higher among the subjects who learned the concepts according to the so-called modern learning method. These subjects, however, were seldom able to verbalize the learning material. Subjects who learned by the traditional method verbalized more frequently than subjects who had learned by the modern method. His results are thus very much akin to our own.

In the work of Katona and Szekely it was clear why some subjects responded in one way to the learning material and other subjects responded in quite a different way. It was because the experimenter imposed a manner of learning upon the subjects. In our experiments, however, subjects were not asked to learn the material at all. It is possible, however, that different 'sets' towards learning existed between our different groups which determined *how* they learnt and what they extracted from the material. These different methods of learning may in turn have determined (as in Szekely's experiments) the final form of response. Let us examine whether individual differences existed that might have accounted for different 'sets' towards learning.

The greatest significant difference in our results was between drivers and non-drivers, the drivers using far more of the information in relation to the amount they recalled than non-drivers. Car-drivers have, in learning to drive, assimilated road safety information in relation to its being incorporated into their car-driving behaviour. These learning periods have in fact been very similar to the activity learning or modern method used by Szekely in his experiment—information was organized and learnt in relation to its being used. It is likely, therefore, that car drivers in repeatedly organizing material in this way during their experience of learning-in-order-to-use and of using are building up a way of apprehending and reacting to information in a particular situation. Thus when the car-drivers are presented with further learning material such as this incidental propaganda the response seems likely to be organized consistently with previous responses. They will in fact have a set towards learning-in-order-to-use—but not necessarily to recall. As Postman & Senders (1946) have suggested, sets not only determine the course of learning but may themselves be the result of previous learning.

Our hypothesis now needs to be considered in the light of the performance of our other groups. We noted that students tended to recall more information in relation to the amount used than the naval ratings. It was also apparent that the older subjects had very poor recall scores, although their use scores were relatively high. (This latter finding is in contrast to the assumption which has generally been made, and which has been based on the results of memory tests, that older subjects learn less well than younger ones.)

Where there has been no previous learning specific to the new learning situation (in this case, road safety regulations), it seems likely that individuals will respond in the way they have been most accustomed to respond in other learning situations. As a product of our educational system, learning information (Szekely's traditional method) and memorizing are likely to be predominant forms of responses. On these assumptions, it is hardly surprising that the student subjects should have a high recall score. They have continuously exercised the 'traditional method' of learning and response. The formal education of the naval ratings, however, ceased at 14 or 15 years of age, since when 'learning by activity' is likely to have been predominant in their training in the navy. Again we can postulate that the older people through their experience over many years have developed preferred persistent responses in a learning situation. This particular group of over-sixties had a low educational attainment and were mostly artisans. Their learning had obviously been orientated more towards 'activity'.

Our hypothesis (which will of course have to be tested in relation to other learning situations before its generality can be assumed) is that the manner in which incidental information is retained (either in memory or for use in relevant action) will be determined at the time of learning and conditioned by previous learned responses. If this is so, then it seems that for propaganda to have the optimum effect on behaviour the individual should previously have organized appropriate behaviour into his learning experience.

Propaganda should in fact be used as a reinforcement of some more active learning. The results also yield implications for educational practice. In the past both teachers and psychologists have focused their attention on the importance of learning by repetition or learning substance. It is clear that these are not always the most effective ways of learning if the application of such learning is the criterion of effectiveness. What seems of even greater importance is that these methods of learning may well become habitual responses and so determine the course and manner of future learning. The teacher who assumes that ability to reproduce learned material is indicative of the ability to apply the information usefully is apparently making a false assumption. What is worse is that he seems to be prejudicing the individual's chances in future learning situations.

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DISPLAY-CONTROL RELATIONSHIPS ON CIRCULAR AND LINEAR SCALES

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The variation in tracking performance between different quadrants of a circular scale has been shown to be attributable mainly to the ambiguity of display-control relationships on this scale, in that the motion of the pointer may be viewed as either rotary or translatory. The dominant tendency is to view it as rotary, a clockwise control movement being associated with a clockwise pointer movement; but there is a significant secondary tendency to view it as translatory, a clockwise control movement being associated with a displacement of the pointer upwards or to the right, according to the quadrant in which the target lies. The two tendencies conflict when the target is in the lower and right-hand quadrants, where performance is consequently poorer. Performance is also affected by position in the visual field as such.

It has been verified that a clockwise control movement is expected to send the pointer to the right on a horizontal linear scale and upward on a vertical linear scale. Performance on the horizontal scale is no better than that obtained on the circular scale under optimal conditions, but the unambiguity of the linear scale renders it preferable where erroneous reactions are to be avoided.

I. Introduction

Workers in the field of equipment design early recognized that the problems of display and control are not independent and, in particular, that, in the frequent case in which the display involves a moving element (such as a pointer), the directional relationship between pointer movement and control movement may become a variable which has a considerable influence on the speed and accuracy of operation. Where, for example, a pointer and a control lever move linearly in the same direction, the operator appears to expect that a given control movement will result in a pointer movement of the same sense ('direct' relationship), and may be disturbed if it results in one of opposite sense ('reversed' relationship).

It has been found that, in many cases, there is such a 'preferred' directional relationship between control and display. Where the task is not too demanding, the preferred relationship may yield no better performance than its opposite (Grether, 1947); but where the task is more stressful, as when it demands speed or division of attention, the directional relationship may become important (Mitchell & Vince, 1951). Such experimental evidence as is available (Mitchell & Vince, 1951) indicates that, with sufficient practice, performance with a non-preferred arrangement may be brought to as good a level as that obtained with a preferred relationship. The lengthier training period may, of course, be something of a disadvantage; but more serious, perhaps, is the fact that although training may succeed in reversing an existing habit, the operator is liable to revert to his earlier reaction under conditions of stress (Simon, 1951). It is therefore the more necessary, where stress is anticipated and the result of a mistake may be serious, to design equipment so that direction-of-motion relationships conform to the existing expectations of the majority of operators.

Some situations are found to be so unfamiliar that no stereotype exists, as when display and control move in different planes (Carter & Murray, 1947; Gardner, 1950; Mitchell & Vince, 1951; Warrick, 1947). These situations are ambiguous in the sense that an untrained operator is equally likely to make either of two opposing reactions. Other situations, such as the circular scale with which we are here concerned, are ambiguous in another sense: namely, that they may be susceptible of more than one interpretation.

II. DISPLAY-CONTROL RELATIONSHIPS ON THE CIRCULAR SCALE

Despite the wide use of circular scales, rather little attention has been paid to the display-control relationships associated with them.

Fitzwater (1948) studied a compensatory pursuit task in which the operator used a control lever to maintain the pointer on the '12 o'clock' mark of a circular scale, from which it was periodically displaced by externally produced disturbances. He found that a significantly better score was obtained when a clockwise movement of the lever resulted in a clockwise rotation of the pointer than when it caused the pointer to rotate counter-clockwise.

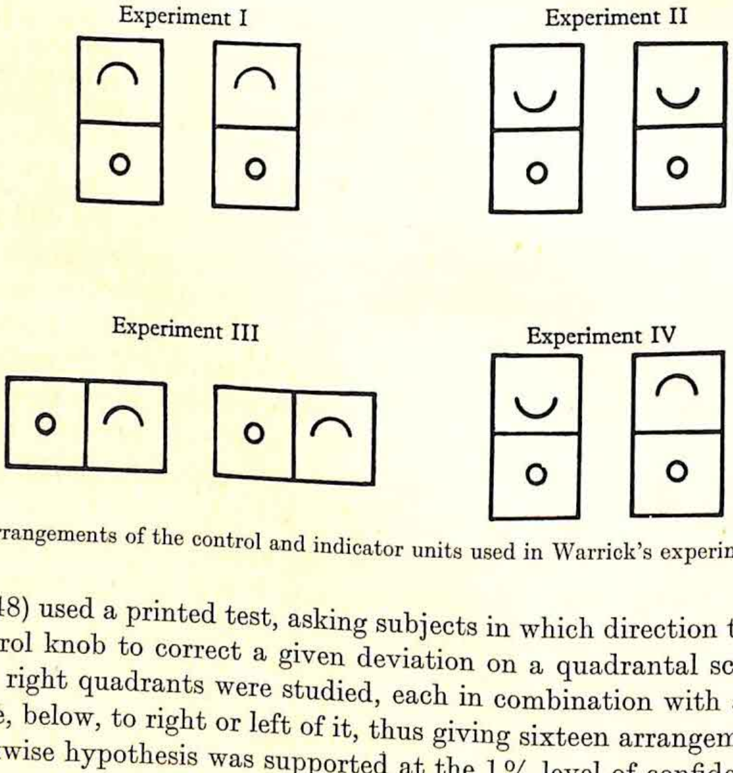


Fig. 1. Arrangements of the control and indicator units used in Warrick's experiment (1949).

Warrick (1948) used a printed test, asking subjects in which direction they would turn a circular control knob to correct a given deviation on a quadrantal scale. The upper, lower, left and right quadrants were studied, each in combination with a control which might be above, below, to right or left of it, thus giving sixteen arrangements in all. The clockwise/clockwise hypothesis was supported at the 1% level of confidence, differences between the arrangements being insignificant.

In a further study by Warrick (1949), subjects alternately adjusted two semicircular indicators by means of rotary control knobs. The relative position of indicator and control was varied in four experiments, as shown in Fig. 1. Within each experiment, the control-display relationship was varied, direct-direct, reversed-reversed, direct-reversed, and reversed-direct combinations (*DD*, *RR*, *DR* and *RD*) being presented in balanced order.

The results from the first three experiments showed that when two indicator-control units with like arrangements are operated alternately, operators perform more rapidly and consistently if the motion relationships of the units are the same—that is, the *DD* and *RR* combinations are superior to the *DR* and *RD*. Moreover, a difference was found between the consistent combinations, direct drive being superior to reversed (*DD* to *RR*). The clockwise/clockwise hypothesis is thus further supported.

The fourth experiment, in which the panel arrangements were unlike, yielded rather different results. *DD* was no better than *DR* and *RD*, while *RR* was much worse. Warrick's interpretation is that when panel arrangements are unlike, consistent motion-relationships do not help, and none of the relationships are as effective as when panel arrangements are alike.

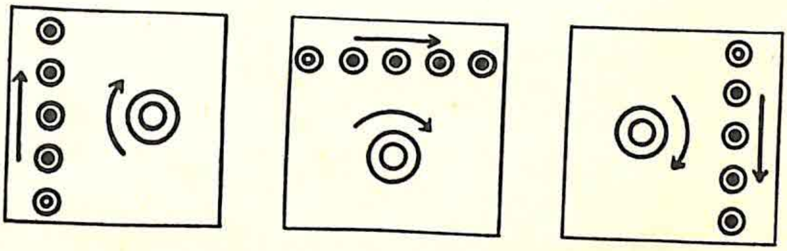


Fig. 2. Response preferences determined by Warrick (1947).

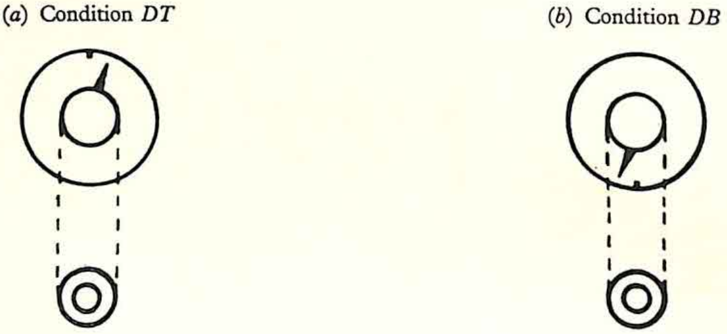
Murrell (1952) comments that Warrick's interpretation of his findings appears to be influenced by a general principle of control-display relationships which he enunciated as the conclusion of a previous experiment using linear scales (Warrick, 1947), namely, that a pointer is expected to move in the same direction as that part of the control nearest to the display (see Fig. 2). This principle, when carried over to the most frequently used upper segment of a circular dial, reinforces the common-sense assumption that the expected relationship between a knob and a circular dial is clockwise/clockwise (Fig. 3*a*). But Murrell suggests that in the bottom segment of the scale, the expected relationship will not necessarily be clockwise/clockwise; this would, in fact, be contrary to the first conclusion above, since the pointer would then move in the opposite sense to the nearest part of the control (Fig. 3*b*). He points out that in Warrick's Exp. IV, the best arrangement was in fact *RD*, in which a right-left pointer movement in the bottom half of the dial corresponds with a right-left (anticlockwise) movement of the upper part of the control.

Murrell elsewhere (1951) puts forward his own principle of display-control relationships. Research, he says 'has established beyond reasonable doubt that when a circular dial and control are in the same plane the operator expects the pointer to move as if it were pivoted at the centre of the control'. The point of this formulation is evidently to include a reversal in the bottom segment of the circular scale.

Such a reversal is not, however, established 'beyond reasonable doubt'. The difference in Warrick's Exp. IV is not significant, and cannot in any case be considered uninfluenced by the alternation of tasks. It is, however, true that Warrick ignores the possible significance of unequal performance in different segments of the scale, even though there is some suggestion of this in his own Exps. I and II. Evidence of such irregularities would require modification of the clockwise/clockwise hypothesis.

Graham (1952), using a compensatory pursuit task with a control knob below the dial, found differences in performance with choice of target. Speed and accuracy were better when the target was at the top of the scale than when it was at the bottom, right or left cardinal points. She suggested that this might be due to an ambiguity in the control-display relationships.

Similarly, Fitts & Simon (1952), in a dual pursuit task, found performance to be superior in the top and left-hand quadrants of the dials. They point out that similar differences have been found in other types of investigation. Dallenbach (1923) found that lights appeared brighter in these quadrants than when below or to the right of the fixation point. Eye fixations have been found to be most frequent in this area. Connell & Grether (1948) and White (1949) have shown that dials are read more quickly and accurately in these sectors.

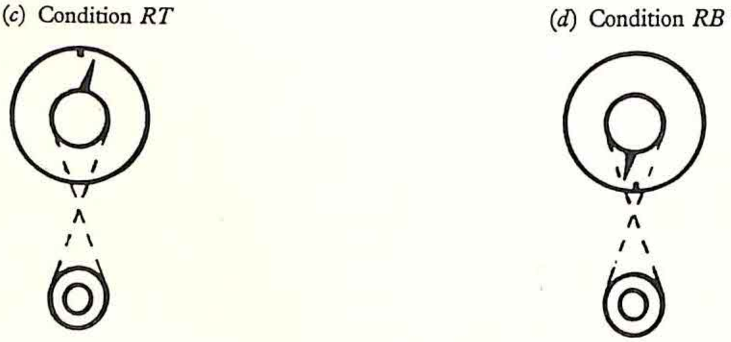


(*a*) Condition *DT*

pointer moves right and clockwise
control clockwise increases error
control to right increases error

(*b*) Condition *DB*

pointer moves left and clockwise
control clockwise increases error
control to left decreases error



(*c*) Condition *RT*

pointer moves right and clockwise
control clockwise decreases error
control to right decreases error

(*d*) Condition *RB*

pointer moves left and clockwise
control clockwise decreases error
control to left increases error

Fig. 3. Display-control relationship on the circular scale.

It is not clear how far, if at all, Fitts & Simon consider that such a factor of position in the visual field might account for their own results. Certainly they put forward an alternative explanation, that 'direction-of-motion cues are ambiguous when rotary motion occurs in these (bottom and right) sectors. In order to account for this ambiguity, we assume that a moving stimulus object, such as an instrument pointer, is responded to in accordance with either a "rectangular-coordinate hypothesis" or a "polar-coordinate hypothesis". If we assume that the two hypotheses lead to the same motor or verbal response for a specified angular position of the pointer, then the two hypotheses will lead to opposite responses when the pointer has passed through an 180° rotation (i.e. on the reverse side of the dial). As an illustration, a clockwise rotation at 9 o'clock is "up" and at 3 o'clock is "down", according to the rectangular hypothesis'.

Fitts & Simon are careful to stress that the ambiguity of a display-control relationship may be affected by the type of control used and its placement—for example, when a lever is used instead of a rotary control, its orientation will be important (Simon, 1952). Again, the ambiguity effect may well be enhanced by the use in their experiment of two spatially separated pointers. But they feel that, since only operators responding in accordance with the rectangular hypothesis would show a poorer level of performance in the 3 o'clock and 6 o'clock positions, 'this provides indirect evidence that most of our subjects must have responded at times on the basis of this hypothesis, even though the displays and controls provide a rotary or polar-coordinate system'.

III. ANALYSIS OF PERFORMANCE ON A CIRCULAR SCALE

It is suggested, then, that in the common case in which the control is a knob in the same plane as the display, the display-control relationship may be ambiguous, in that the operator may regard the pointer movement either as rotary, or, over a limited segment of the scale, as translatory. If he acts on the first hypothesis, then he may, for example, expect a clockwise control movement to move the pointer clockwise; whereas, if he acts on the second assumption, he may expect it to move to the right. When the target is at the top of the scale, both expectations lead to the same response; but, at the bottom of the scale, they lead to antagonistic response tendencies, conflict between which will be reflected in poorer performance.

It is clear that those differences in performance in different sectors of the circular scale which have so far been reported may be accounted for along these lines. Unfortunately, they may equally well result simply from the different locations of the target within the visual field, an hypothesis which finds some support in the observations quoted by Fitts & Simon. The experimental evidence to date does not allow us to decide between these two explanations. Such a decision requires an examination of performance on diametrically opposite targets and with both directions of drive. This may be seen from the following analysis.

Suppose that tracking error scores are obtained under four conditions: with targets at the top and bottom of the scale, in each case with both direct and reversed drive. Let the four scores be denoted by *DT*, *RT*, *DB*, *RB*.

It may be supposed that these scores will be affected by the two direction-of-motion expectations suggested above. Let the scores be presumed to contain components 'r' and 't' corresponding to the 'rotary' and 'translatory' reaction tendencies. The scores may also be affected by choice of target, in that performance at the top of the scale may be inherently more easy or difficult than at the bottom. Let there be a component 'p' corresponding to this position factor. Finally, the scores will be affected by a number of constant factors whose effects will be included in the general overall mean 'm'.

Any particular score may then be regarded as the resultant of four components:

m the general overall mean.

p the position term, arising from differences between targets as such, and defined as positive if the top target produces the greater error.

r the term arising from the 'rotary tendency', defined as positive when a clockwise displacement of the pointer evokes a clockwise correction.

t the term arising from the 'translatory tendency', defined as positive when a displacement of the pointer to the right evokes a clockwise correction.

Now, under the condition DT (Fig. 3*a*), the components 'r' and 't' will operate in the same direction. For example, the pointer moves to the right (clockwise). If 'r' is positive, the operator will tend to move the control clockwise, so increasing his error. Similarly, if 't' is positive, he will again tend to move the control clockwise, increasing his error score. Hence the score under condition DT is given by

$$DT = m + p + r + t.$$

Conversely, under the condition RT (Fig. 3*c*), positive 'r' and 't' components both operate to decrease the scores, whence

$$RT = m + p - r - t.$$

On the bottom target, however, 'r' and 't' become antagonistic. When direct drive is used (Fig. 3*b*), positive 'r' increases the error and positive 't' decreases it, so that

$$DB = m - p + r - t.$$

And when the drive is reversed (Fig. 3*d*),

$$RB = m - p - r + t.$$

Solution of these equations gives

$$\begin{aligned} m &= \tfrac{1}{4}\,(DT + RT + DB + RB), \\ p &= \tfrac{1}{4}\,(DT + RT - DB - RB), \\ r &= \tfrac{1}{4}\,(DT - RT + DB - RB), \\ t &= \tfrac{1}{4}\,(DT - RT - DB + RB). \end{aligned}$$

Thus, from such a set of scores, we may estimate the strength of these components. The statistical significance of the last three factors may be estimated from the 'between targets', 'between drives', and 'drives × targets interaction' terms of an analysis of variance of the scores.

Experiments which allow of such analysis should indicate what factors are needed to account for observed variations of performance in different sectors of a circular scale.

IV. EXPERIMENT ONE: UPPER AND LOWER QUADRANTS

This experiment was designed to permit the analysis described above; that is, a comparison was made of performance on targets at the top and bottom cardinal points of a circular scale, with both directions of drive.

The task used was compensatory tracking. The display consisted of a scale 2·75 in. in diameter, with 10 × 10 white-on-black graduations. Vertically below it was a control knob 2·5 in. in diameter, connected through a differential gear to the pointer. The second input to the differential gear was obtained from a cam follower, which produced step-type disturbances at an average rate of one every 2·1 sec. The subject's task was to counteract these disturbances so as to maintain the pointer on a given target. A voltage proportional to the deviation of the pointer from the target was obtained from a potentiometer, and integrated against time by an electronic device. The score so obtained was the 'tracking error', measured in arbitrary units. The scores were subsequently normalized by a logarithmic transformation.

The subjects were tested individually. Before the test, the subject was allowed to handle the control knob for 15 sec. to discover the display-control relationship. This was followed by 60 sec. practice. The test run lasted 90 sec., but the first and last 15 sec. were not scored.

The subjects were seventy-two aircraftmen of the Royal Air Force, their ages ranging from 18 to 26 years. All had visual acuity of 20/20 or better as tested by the Snellen chart. They were divided into groups of eighteen subjects, each group being tested under one of the four experimental conditions.

The mean scores obtained under the four conditions, estimates of the factors presumed to account for these scores, and of the significance of these factors as estimated by an analysis of variance, are shown in Table 1. Tests of the significance of differences between means show that the error score obtained under condition *DT* is smaller than that obtained under the other conditions; no other differences are significant at the 5 % level of confidence. All the terms of the analysis of variance are significant at this level.

Table 1. *Mean scores, score components, and analysis of variance—Exp. I*

(*a*) Mean Scores

		Differences			
	Means	*DT*	*DB*	*RT*	*RB*
DT	154·1	—	14·8*	20·8*	21·2*
DB	168·9	—	—	6·0	6·4
RT	174·9	—	—	—	0·4
RB	175·3	—	—	—	—

* Significance at 5 % C.L.

(*b*) Score components

$m = 168{\cdot}30$
$p = -3{\cdot}80$
$r = -6{\cdot}80$
$t = -3{\cdot}60$

(*c*) Analysis of variance

Source of variation	Sums of squares	Degrees of freedom	Mean squares	*F*	Significance (5 % level)
Between drives	3335	1	3335	17·28	*
Between targets	1043	1	1043	5·40	*
Drives × targets	953	1	953	4·98	*
Residual	13138	68	193	—	—
Total	18469	71	—	—	—

It will be seen that although both direction-of-motion factors are present, '*r*' appears to be the dominant tendency—that is, subjects react primarily to angular motion of the pointer. There is, however, a significant secondary tendency to respond in terms of 'left' and 'right', a clockwise control movement being expected to move the pointer to the right. Finally, there is a tendency for performance at the top of the scale to be inherently superior to that at the bottom.

V. Experiment two: left-hand and right-hand quadrants

This experiment involved a comparison of performance on targets at the left-hand and right-hand cardinal points of the scale, again with both directions of drive, so that the four experimental conditions were *DR*, *DL*, *RR*, *RL*. In all other respects the design of the experiment was identical with that first described. The results are presented in Table 2.

The variance of *DR* is significantly smaller than that for the other three conditions, and the differences between means have therefore been tested by Behren's method (Fisher & Yates, 1948). The difference between *RL* and *RR* is not significant at the 5% level of confidence. *DR* is superior to either of the reversed drive conditions, and *DL* to all other conditions. The analysis of variance shows that the 'between drives' and 'drives × targets interaction' terms are significant at the 5% level.

Table 2. *Mean scores, score components, and analysis of variance—Exp. II*

(*a*) Mean scores

		Differences			
	Means	*DL*	*DR*	*RL*	*RR*
DL	154·7	—	8·0*	29·7*	24·3*
DR	162·7	—	—	21·7*	16·3*
RL	184·4	—	—	—	5·4
RR	179·0	—	—	—	—

* Significance at 5% C.L.

(*b*) Score components

$m = 170{\cdot}20$
$p = -0{\cdot}65$
$r = -11{\cdot}50$
$t = -3{\cdot}35$

(*c*) Analysis of variance

Source of variation	Sums of squares	Degrees of freedom	Mean squares	*F*	Significance (5% level)
Between drives	9499	1	9499	47·50	*
Between targets	31	1	31	0·15	—
Drives × targets	807	1	807	4·08	*
Residual	13447	68	198	—	—
Total	23784	71	—	—	—

Both direction-of-motion factors are thus present, and the 'rotary tendency' again appears to be dominant, a clockwise control movement being expected to move the pointer clockwise. But again there is a significant secondary tendency, this time to respond in terms of 'up' and 'down', a clockwise control movement being expected to move the pointer upwards.

It will be seen that there is no evidence in this experiment for an inherent superiority of the left-hand target. Differences in scores are attributable solely to the direction-of-motion factors.

VI. Discussion on the circular scale

It appears then that differences in performance on the circular scale can be accounted for by three factors:

(*a*) A general tendency for performance to improve as the target is moved from the bottom to the top of the scale. The reason for this is uncertain. It is possible that the position of the target relative to the subject's eye-level is involved; but if so, this variable is more important than has generally been supposed. The evidence cited above predicted an improvement from right to left also; this did not appear in the present results.

(*b*) An expectation that a clockwise control movement will result in a clockwise pointer movement. This finding is supported by other evidence.

(*c*) An expectation that a clockwise control movement will move the pointer to the right or upwards, according to the target used. The movement to the right conforms to Warrick's principle that a pointer is expected to move in the same direction as that part of the control nearest the display. It also agrees with what is known of direction-of-motion relationships on the horizontal linear scale. In respect of the tendency to expect the horizontally aligned pointer to move upwards, it would seem desirable to seek confirmation in the case of a vertical linear scale with control knob beneath it; no evidence bearing on this case has been found in the literature.

VII. EXPERIMENT THREE: LINEAR SCALES

This third experiment was therefore intended primarily to determine the optimal direction-of-motion relationship on a vertical linear scale. The optimal relationship on a horizontal linear scale was not in doubt, but it was decided to include such a scale also in order to secure comparative measures of performance. There were thus four experimental conditions, according as a clockwise control movement displaced the pointer upward, downward, to right or to left (conditions U, D, R, L). The length of the scale was equivalent to that of the circumference of the circular scale; the target was at its central point. In other respects the experimental design was the same as in the first two experiments. The results are shown in Table 3.

Table 3. *Mean scores, score components and analysis of variance—Exp. III*

(*a*) Mean scores

		Differences			
	Means	*D*	*U*	*L*	*R*
D	174·1	—	11·1*	4·2	20·1*
U	163·0	—	—	15·3*	9·0*
L	178·3	—	—	—	24·3*
R	154·0	—	—	—	—

* Significance at 5% C.L.

(*b*) Score components

Vertical scale: $m = 168{\cdot}6$
$t = -5{\cdot}6$
Horizontal scale: $m = 166{\cdot}2$
$t = -12{\cdot}2$

(*c*) Analysis of variance

Source of variation	Sums of squares	Degrees of freedom	Mean squares	*F*	Significance (5% level)
Between scales	103	1	103	0·66	—
Between drives	5653	1	5653	36·47	*
Drives × scales	800	1	800	5·16	*
Residual	10514	68	155	—	—
Total	17070	71	—	—	—

The scores for the different conditions differ significantly in variance, and allowance has been made for this in testing the differences between means. The difference between *D* and *L* is not significant at the 5% level of confidence. *U* is superior to both these conditions, and *R* to all other conditions. This verifies the predicted association of a clockwise control movement with an upward pointer movement on the vertical scale and a movement to the right on the horizontal scale; performance under the latter condition is superior to that under the former. The analysis of variance shows that this is due to a stronger association of display and control movements on the horizontal scale rather than to any difference in mean performance on the two scales.

VIII. General Discussion

Measures of speed and accuracy of tracking have now been obtained under both display-control relationships for horizontal and vertical scales and for targets at the four cardinal points of the circular scale. The results are brought together in Table 4, where the different arrangements are arranged in order of magnitude of the means. The variances are just significantly heterogeneous at the 5% level of confidence, and appropriate allowance has been made for this in testing the significance of differences between means.

Table 4. *All scales: means, variances and differences between means*

				Differences between means										
Scale	Condition	Mean	Variance	*DT*	*DL*	*DR*	*U*	*DB*	*D*	*RT*	*RB*	*L*	*RR*	*RL*
Horizontal	*R*	154·0	57·6	0·1	0·7	8·7*	9·0*	14·9*	20·1*	20·9*	21·3*	24·3*	25·0*	30·4*
Circular	*DT*	154·1	129·0	—	0·6	8·6*	8·9*	14·8*	20·0*	20·8*	21·2*	24·2*	24·9*	30·3*
Circular	*DL*	154·7	152·9	—	—	8·0*	8·3*	14·2*	19·4*	20·2*	20·6*	23·6*	24·3*	29·7*
Circular	*DR*	162·7	65·0	—	—	—	0·3	6·2	11·4*	12·2*	12·6*	15·6*	16·3*	21·7*
Vertical	*U*	163·0	79·4	—	—	—	—	5·9	11·1*	11·9*	12·3*	15·3*	16·0*	21·4*
Circular	*DB*	168·9	139·0	—	—	—	—	—	5·2	6·0	6·4	9·4*	10·1*	15·5*
Vertical	*D*	174·1	330·6	—	—	—	—	—	—	0·8	1·2	4·2	4·9	10·3
Circular	*RT*	174·9	274·2	—	—	—	—	—	—	—	0·4	3·4	4·1	9·5
Circular	*RB*	175·3	230·7	—	—	—	—	—	—	—	—	3·0	3·7	9·1
Horizontal	*L*	178·3	150·8	—	—	—	—	—	—	—	—	—	0·7	6·1
Circular	*RR*	179·0	266·3	—	—	—	—	—	—	—	—	—	—	5·4
Circular	*RL*	184·4	291·1	—	—	—	—	—	—	—	—	—	—	—

* Significance at 5% C.L.

The results of this test suggest that the twelve arrangements investigated may be classified into three groups in the following order of decreasing merit:

(A) Horizontal linear scale, clockwise control movement sending pointer to right.

Circular scale, target at top, clockwise control movement sending pointer clockwise and to right.

Circular scale, target at left, clockwise control movement sending pointer clockwise and upwards.

(B) Vertical linear scale, clockwise control movement sending pointer upwards.

Circular scale, target at bottom, clockwise control movement sending pointer clockwise and to left.

Circular scale, target at right, clockwise control movement sending pointer clockwise and downwards.

(C) Arrangements with control display relationships the opposite of above.

We have accounted for the way in which performance on the circular scale varies with the position of the target by stressing the ambiguity of control-display relationships on this scale; this ambiguity increases the danger of the control being turned in a direction which increases rather than decreases the error. Observations of transfer effects suggest that this danger is greater when the target position varies on the same or different instruments. If, in view of these considerations, the effective working area of the circular scale is reduced, the advantage of this scale in wrapping '10 in. of scale around a 3 in. dial' appears less impressive.

It is therefore suggested that although the three arrangements listed in group (A) above lead to an equally good mean performance, the horizontal linear scale may well be preferred, especially when erroneous control movements are to be avoided. It will be observed in this connexion that performance on this scale is more consistent than on any other.

Similar considerations apply to the position of the vertical scale in group (B).

It must be emphasized that the above conclusions are limited to tracking tasks in which the control knob is vertically below the display. It would be desirable to investigate a situation in which the knob is in some other position, for example, to the right of the display. In the case of the linear scales the optimum control-display relationships are probably the same, but it may well be found that performance on the vertical scale is now superior to that on the horizontal.

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SOME STUDIES OF PERCEPTION IN RELATION TO AGE

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A series of experiments to discover the extent of age differences in visual perception when temporal integration was required suggested that:

(1) Where the displays were simple, the performance of different age groups was nearly equal, but with increasing complexity of displays the older subjects were relatively increasingly slow in making correct identifications.

(2) With the complex displays there was a relationship between the degree of correct identification and (*a*) the cumulative viewing time; (*b*) the amount of the display seen at one moment, such that an increase in both led to a greater improvement in the performance of the older subjects than an increase in either time or amount alone. Given a sufficient increase, the performance of older subjects equalled that of younger.

(3) The greater difficulty of older subjects with complex material appeared to be due mainly to the greater amount of temporal integration required with such material.

I. Introduction

Previous studies of perceptuo-motor performance in relation to age have suggested that perceptual rather than motor factors often set the limits to performance among older people, and that this limit is not due to falling efficiency of the peripheral sense organs so much as to central changes (Welford *et al.* 1951). An experiment by Bernardelli (see Welford *et al.* 1951) suggested that one of the difficulties for older people lies in the relating together of information arriving serially in time—a finding which would seem to be in line with much clinical evidence about senile conditions, although not commonly recognized as a feature of normal middle or early old age.

Accordingly, displays were presented in such a way that only a small portion was visible at any one instant, but the whole was successively revealed. Correct identification of the displays depended upon integrating material perceived serially over a period of 2–3 sec.

The apparatus consisted of a box containing a funnel 15 in. long and 6 in. deep into which the subject looked through an aperture 5 in. across and 1½ in. deep. At the end of the funnel was a horizontal slit, which could be varied in depth, directly in front of, and the width of, a well-illuminated band. The displays were attached to the band and seen through a transparent screen, and the band moved downwards behind the slit, the lowest portion of the displays being thus the first to appear.

The displays to be identified were boldly drawn in black ink on a white ground. They had previously been used in experiments by Dr M. D. Vernon. The subject was given warning of the approach of the display by two lines on the band which formed a Λ and appeared in the slit as two black spots which converged gradually towards the centre, reaching it just before the display appeared.

It was not possible to test the subjects' vision before the experiments, but all subjects who normally used glasses wore them, and subjects were questioned to ensure that they could see the displays properly.

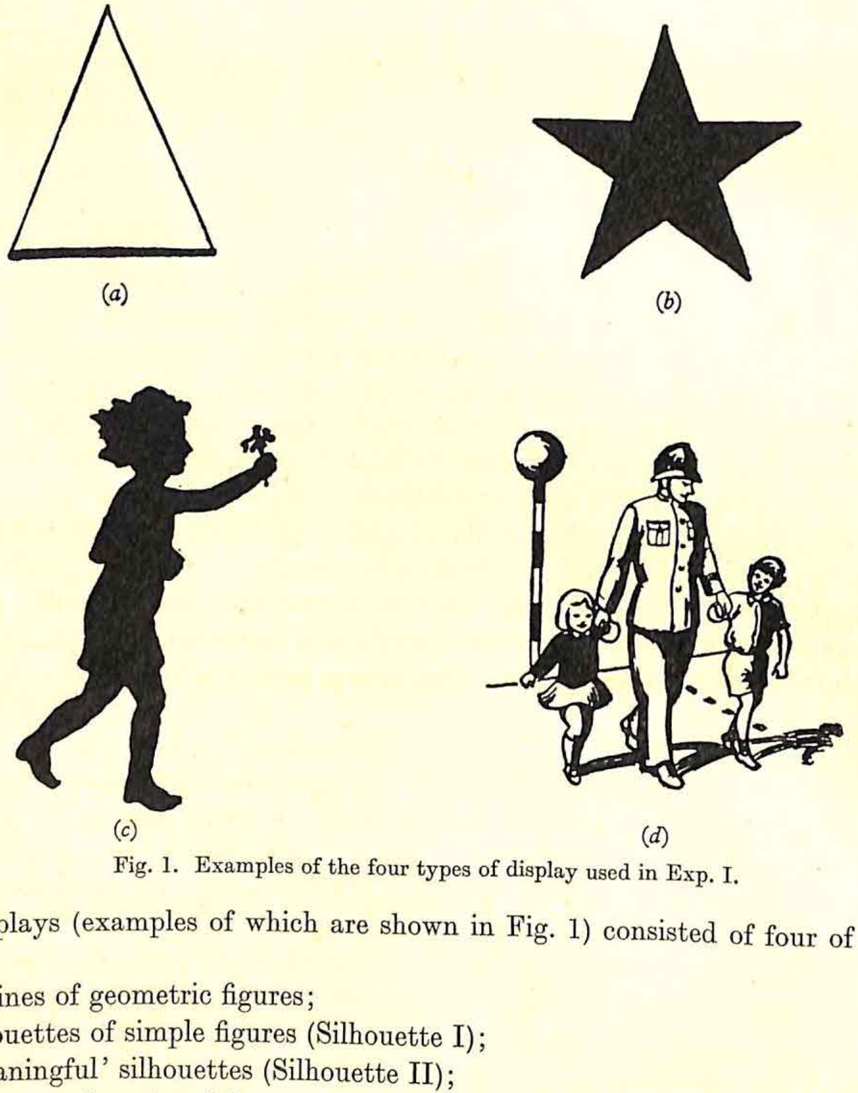
Subjects were told to report verbally what they had seen, in as much detail as possible, immediately after each presentation.

II. PROCEDURE AND RESULTS

A. *Varying complexity in relation to age*

Experiment I

In this experiment a comparison was made of the performance of two age groups with displays having various degrees of complexity. The groups consisted of sixteen subjects aged under 30 and sixteen subjects aged over 60. The younger group were all naval ratings with an average age of 19·7 years. The older were mostly members of an Over Sixties Club, of both sexes, with an average age of 71·7.



(*a*) (*b*) (*c*) (*d*)

Fig. 1. Examples of the four types of display used in Exp. I.

The displays (examples of which are shown in Fig. 1) consisted of four of each four types:

(*a*) outlines of geometric figures;
(*b*) silhouettes of simple figures (Silhouette I);
(*c*) 'meaningful' silhouettes (Silhouette II);
(*d*) more complex pictorial representations.

Two of each of the above types were shown under two different conditions, at a band speed of 1 in. per sec.:

(1) Each picture was shown first with the horizontal slit 0·1 in. deep. If the subject

failed to identify the picture correctly after one presentation at 0·1 in. he was shown it once at 0·2 in. and so on to 0·5 in., or to the point of identification if sooner.

(2) Each picture was shown at one aperture throughout—half of them at 0·1 in. and half at 0·3 in.—up to 3 times or until correct identification, whichever came sooner. Pictures were shown alternately at 0·1 and 0·3 in. for each subject. This condition was introduced as a control in order to discover how much improvement in recognition could be brought about merely by repetition of the display without widening of the aperture. The results will be given later when a number of controls are being discussed.

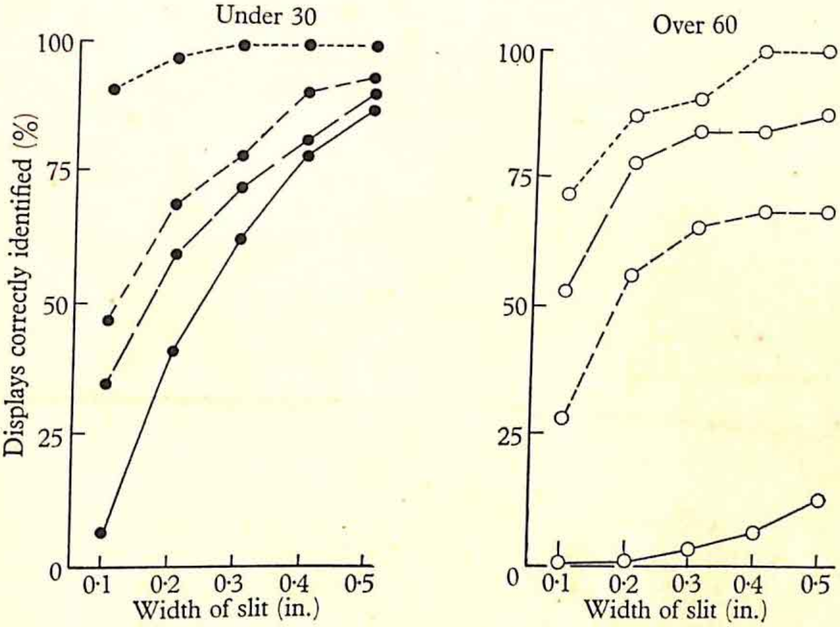


Fig. 2. Progress of correct identification as aperture increased. Exp. I. *Key*: Geometric Figures, dotted line; Silhouette I, large dashes; Silhouette II, small dashes; Pictorial Representations, black line.

Each subject saw eight of the pictures under one condition and eight under the other, and the pictures were seen an equal number of times under each condition by each age group. The order of presentation was varied within each group.

Results with increasing aperture (*Condition* 1)

The numbers of correct identifications following each presentation are shown in Fig. 2. The two age groups gave very similar results for the geometric figures. The older were a little less successful than the younger in identifying the silhouettes, and strikingly less successful with the pictorial representations. The differences in number of successes with age are shown in Table 1. There thus appears a transition from near equality between the age groups with the easiest material to wide divergence with the more difficult. This is in line with the results of several perceptuo-motor experiments where a similar transition has been shown (Szafran in Welford, 1951; Kay, 1954).

The possibility was investigated that the older subjects might be having greater difficulty because of inability to identify the displays correctly even outside the apparatus. No such inability was found, and even in the apparatus at the widest aperture possible ($1\frac{4}{10}$ in.), which was not wide enough to show any picture in entirety at one moment, failures to identify completely on one presentation only amounted to 17%.

Table 1. *Number of successes and analysis of failures, after five presentations. Exp. I, Increasing Aperture*

Type of display	No. of successes (max. 32)	No. of failures		No. failing
		1	2	
	Under 30			
Geom. Figs.	32	.	.	0
Sil. I	30	2	.	2
Sil. II	29	3	.	3
Pict. Repns.	28	4	.	4
				9
	Over 60			
Geom. Figs.	32	.	.	0
Sil I	22	6	2	8
Sil. II	28	4	.	4
Pict. Repns.	5	3	12	15
				41

Comparison of successes of under thirties and over sixties. (τ)*

Geom. Figs.	No correlation
Sil. I	N.D. $=2{\cdot}09$, $P= <0{\cdot}04$
Sil. II	No correlation
Pict. Repns.	N.D. $=4{\cdot}35$, $P= <0{\cdot}001$

* See Kendall, (1948).

B. *Course of age change in perception of complex material and its relation to occupational level*

Experiment II

In this experiment the aim was to investigate in greater detail the course of the age change in the perception of complex material and the relation of this to occupational level. To shorten the experiment only 'pictorial representation' displays were shown—the four used in Exp. I and four others of the same type.

Each subject saw half the pictures under one condition, half under another, the two conditions being:

(1) As in Exp. I, starting at 0·1 in. for the first presentation and rising by 0·1 in. on each subsequent presentation up to 0·5 in. or to correct identification if sooner.

(2) At a fixed aperture of 0·5 in. up to five presentations or correct identification if sooner. The second condition was again a control, the results from which will be considered later.

The subjects were divided into two groups according to occupational level. Group 1 consisted of naval ratings, men employed at a Government Training Centre and pensioners of a factory employing mainly unskilled and semi-skilled workers, and were comparable in level to the subjects of Exp. I. Group 2 consisted of University lecturers and research workers, those in administrative positions in industry and others between these two levels.

Each group consisted of eight subjects in each decade from the twenties to the sixties. There were also eight in group 1 in their seventies whose results are included wherever they do not affect comparison as between equal numbers of subjects.

Results with increasing aperture (*Condition* 1)

Numbers of correct identifications are set out for each age decade and for each occupational group in Fig. 3. The results are not entirely regular, but it can be seen that broadly speaking there is little difference between the twenties, thirties and forties in either occupational group, but a substantial and significant difference between these and the subjects over 50. The differences in successes are shown in Table 2.

Table 2. *Number of successes and analysis of failures after five presentations. Exp. II, Increasing Aperture*

Age	No. successes (max. 32)	No. failures 1	2	3	4	No. failing (max. 8)
			Group 1			
21–30	30	.	1	.	.	1
31–40	20	3	.	3	.	6
41–50	23	1	1	2	.	4
51–60	17	3	1	2	1	7
61–70	10	1	1	5	1	8
71–80	9	1	3	.	4	8
			Group 2			
21–30	26	4	1	.	.	5
31–40	26	1	1	1	.	3
41–50	31	1	.	.	.	1
51–60	26	6	.	.	.	6
61–70	10	1	1	5	1	8

Comparison between number of successes of over and under fifties (τ)*

Group 1 (without 70's):
N.D. = 4·83, $P = 0{\cdot}001$.

Group 2:
N.D. = 5·15, $P = 0{\cdot}001$

* See Kendall (1948).

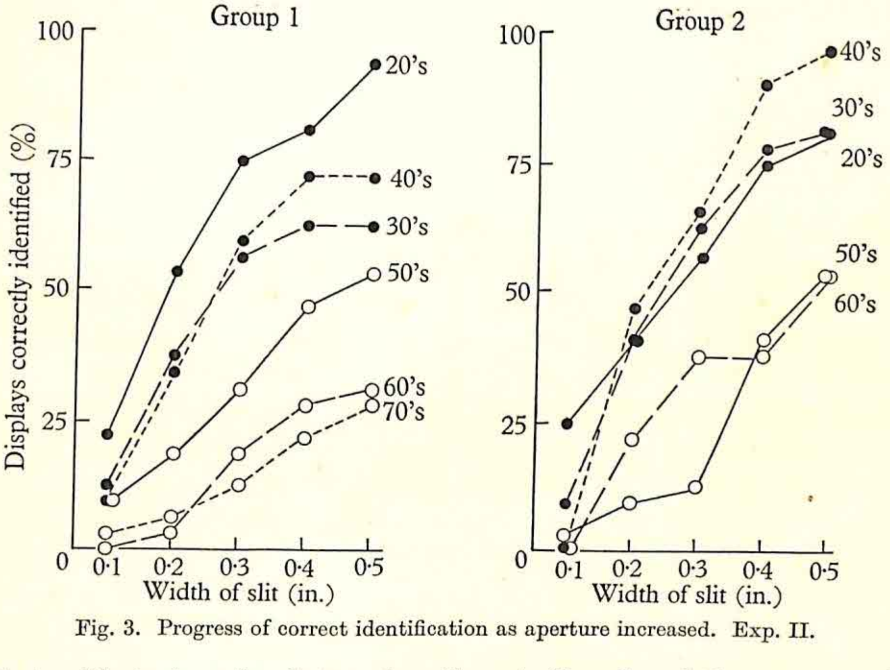


Fig. 3. Progress of correct identification as aperture increased. Exp. II.

The oldest subjects do rather better than those in Exp. I, and the younger subjects do so also on the first presentation. This is probably due to the displays presented in Exp. II being all of one type (i.e. pictorial representations), which meant that the subjects were able to approach the presentation of each new display with a smaller range of possibilities in mind.

The differences between the two occupational groups were small, but, except in the twenties, numbers of successes were in each age range greater among the subjects of higher occupational level.

The content of all the responses was analysed to discover in particular whether failures at identification by older subjects was due to an immediate, but incorrect, interpretation of the picture or to not giving any interpretation. Results are given in Table 3. It was found that there was a slight rise over 50 and a greater one over 60 in the percentage of responses which were either vague and unstructured or where no attempt was made at any interpretation. But the percentage of responses which involved some definite, concrete interpretation of the display never dropped below 64% (the seventies). Beyond such an analysis as this, it was not possible to gain any further information from the responses themselves as to the nature of the difficulty of the older subjects, except as regards repetition of errors, which will be mentioned later.

Table 3. Type of incorrect or incomplete response given. Expt. II, Increasing Aperture

Age	No idea	%	Indefinite	%	Definite	%	Total
			Group 1				
21–30	0	0	2	3·6	53	96·4	55
31–40	1	1·2	2	2·4	83	96·4	86
41–50	2	2·5	6	7·4	73	90·1	81
51–60	4	3·6	11	9·9	96	86·5	111
61–70	26	19·4	11	8·2	97	72·4	134
71–80	27	19·7	22	16·1	88	64·2	137
			Group 2				
21–30	2	2·8	8	11·3	61	85·9	71
31–40	5	6·8	2	2·7	66	90·5	73
41–50	4	6·2	3	4·7	57	89·1	64
51–60	7	5·8	15	12·5	98	81·7	120
61–70	11	9·8	12	10·7	89	79·5	112

Using a less strict criterion for correct identification it was found that the age differences remained the same relative to each other, but that the stricter criterion was more useful for a more detailed investigation of the differences.

The nature of the difficulty involved

The method used in Exps. I and II of presenting a display several times at gradually increasing aperture has the disadvantage that several variables in the display conditions are changed at the same time, and it is not immediately possible to say which is the cause of any difficulty shown by the subjects.

In particular, where the number of displays correctly identified rises with successive presentations, the increase might at first sight be due to any one of three factors:

(*a*) Mere repetition of the exposure.

(*b*) The fact that as the slit widens each part of the display is exposed for a longer time.

(*c*) The fact that widening of the slit results in a larger section of the display being observable at any one time.

The psychological significance of (*b*) follows from the postulate by Hick (1952) that the rate of gain of information is constant, thus relating time to difficulty of identification. The significance of (*c*) is twofold. In the first place, the better articulation of the display as the slit is widened (and more is seen at once) will reduce the amount of information that has to be extracted from the display for identification to be achieved. Secondly, when the aperture is made very wide, or the whole picture is seen at once, there is no need for temporal integration of the material viewed as it passes the slit.

C. *Effects of repetition*

Results with fixed apertures in Experiments I and II

The numbers of correct identifications for the exposures at 0·1 and 0·3 in. in Exp. I are set out in Table 4, and for the exposures at 0·5 in. in Exp. II in Table 5. Comparing these with the corresponding results for increasing aperture it can be seen at once that repetition *alone* is not a sufficient explanation of the rise in the number of correct identifications under increasing aperture conditions. If it were, we should expect the first exposures under fixed aperture and increasing aperture conditions to be the same, and that the two types of condition would keep in step with the second and subsequent exposures. Clearly this does not happen: the numbers of correct recognitions at 0·1 in. after the first are too low, while the numbers of correct recognitions on the early exposures at 0·3 and 0·5 in. are too high.

Table 4. *Number of correct identifications at each presentation. Expt. I*

	Fixed aperture 0·1 in.				Fixed aperture 0·3 in.			
	1	2	3	No. successes (max. 16)	1	2	3	No. successes (max. 16)
				Under 30				
Geom. Figs.	15	0	0	15	16	0	0	16
Sil. I	10	2	0	12	14	0	0	14
Sil. II	7	2	0	9	13	1	1	15
Pict. Repns.	1	0	2	3	6	4	0	10
Total				39				55
				Over 60				
Geom. Figs.	12	2	0	14	15	0	0	15
Sil. I	7	2	0	9	11	1	1	13
Sil. II	10	4	0	14	11	2	0	13
Pict. Repns.	0	0	0	0	2	0	0	2
Total				37				43

Table 5. *Number of correct identifications at each presentation.*
Exp. II, fixed Aperture 0·5 *in.*

	Group 1						Group 2					
	Presentation					No. successes (max. 32)	Presentation					No. successes (max. 32)
Age	1	2	3	4	5		1	2	3	4	5	
21–30	25	3	1	0	2	31	26	5	1	0	0	32
31–40	21	5	2	0	1	29	19	7	4	1	0	31
41–50	22	3	4	1	0	30	24	5	1	0	0	30
51–60	12	10	2	1	1	26	17	4	2	2	1	26
61–70	6	6	3	3	1	19	11	3	3	0	1	18
71–80	4	6	2	1	2	15	.	.	.	.	.	.

D. *Effects of exposure time and aperture*

Experiment III. Exposure time reduced to one third that of Experiments I and II

It seemed possible that correct recognition might, to some extent, be a function of the total length of time for which a picture had been seen in each exposure together with any previous exposures. Thus two exposures at 0·5 in. sec. (fixed speed) might be equivalent to a single exposure at 1·0 sec., or the first two exposures at increasing speed—0·1 and 0·2 in.—might together equal a single exposure at 0·3 in. Fig. 4 shows the results for the thirties of group 2 in Exp. II plotted with these cumulative exposure times on the abscissa. On the same graph are plotted also the results from another group of eight subjects of the same age range and occupational grade who repeated Exp. II but with the band moving at three times its previous speed (i.e. 3 in. per sec.)

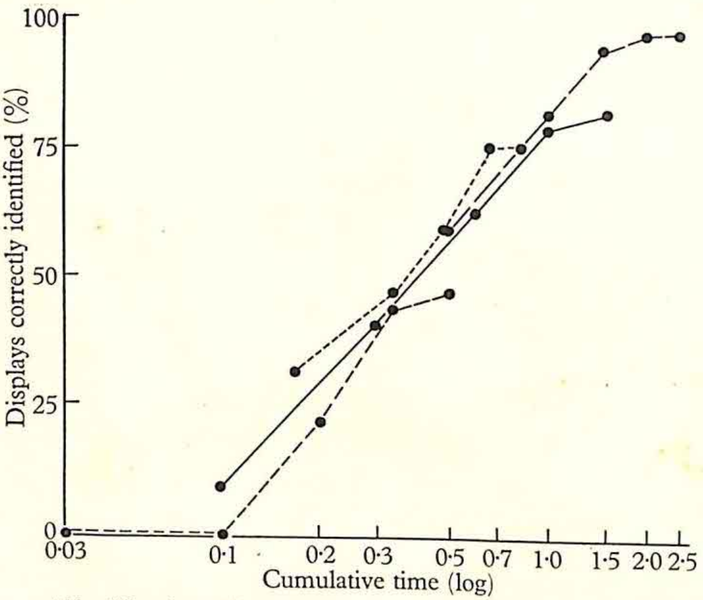


Fig. 4. Progress of correct identification with cumulative time, for the thirties. Exp. II and III. *Key*: Exp. II. Increasing aperture, black line; Fixed aperture 0·5 in., large dashes; Exp. III. Increasing aperture, small dashes; Fixed aperture 0·5 in., dotted line.

If cumulative exposure time were the only important factor the results should all lie along a single line, and there is in fact a close linear relationship between the number correctly identified and the cumulative (log) time, though the results with the 0·5 in. fixed aperture are consistently a little better than those with increasing aperture. It is interesting to note that the relatively high speed of *movement* of the band used in this experiment did not appear to have an adverse effect on performance.

Experiment IV. Increasing exposure time with aperture constant

Fig. 5 shows the results for the youngest and oldest age ranges of group 1 in Exp. II plotted in a manner similar to Fig. 4. Two other sets of results are plotted on the same graph. The first (Exp. IV) is from a group of eight naval ratings aged under 30 and a group of eight members of an Over Sixties Club with an average age of 69·5 who were given the same eight pictures as those used in Exp. II under the following conditions:

(*a*) Four with an aperture of 0·3 in. and the same speed as that used in Exp. I and II (1 in. per sec.), so that any part of the picture was visible for $\frac{3}{10}$ sec.

(*b*) Four with an aperture of 0·3 in. and the speed changing so that the first presentation was such that any part was visible for $\frac{1}{10}$ sec. (equal to the time for which it would be seen with an aperture of 0·1 in. at the speed of 1 in. per sec.), the second presentation so that any part was visible for $\frac{2}{10}$ sec. and so with decreasing speed until it was visible for $\frac{5}{10}$ sec., or until correct identification if sooner. Condition (*b*) thus reproduced the exposure *times* for the increasing aperture condition of Exp. II while keeping the *aperture* constant.

Among the younger subjects the results with 0·5 in. aperture in Exp. II and with both conditions in Exp. IV fit the cumulative time hypothesis reasonably well, but all are superior to the increasing aperture condition of Exp. II. It seems as if the wider apertures of 0·3 and 0·5 in. confer an advantage during the first one or two exposures which persists to subsequent exposures.

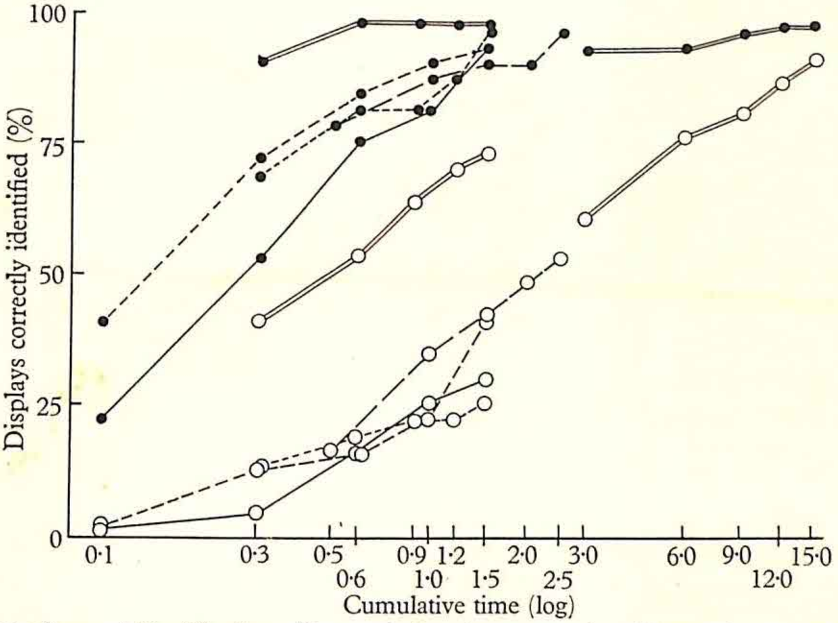


Fig. 5. Progress of correct identification with cumulative time, comparing oldest and youngest groups where aperture and speed conditions varied. Exps. II, IV and V. *Key*: Youngest group, closed circles; Oldest group, open circles. Exp. II. Increasing aperture, black line; fixed aperture 0·5 in., large dashes; Exp. IV. Fixed speed (0·3 in.), dotted line; increasing speed (0·3 in.), small dashes. Exp. V. Tachistoscope, open line.

The older subjects show similar trends although their results are less regular. The results with 0·5 in. aperture from Exp. II are superior to all the others, but the results from Exp. IV are not clearly different from those of the increasing aperture conditions of Exp. II.

It would seem that aperture has some effect over and above the exposure time with which it is confounded in the increasing aperture conditions.

Experiment V. Whole picture shown at once

Confirmation of this last point is obtained from the results of an experiment in which the same eight pictures were exposed in a tachistoscope so that the whole picture was visible at one time for $\frac{3}{10}$ sec. (i.e. the same length of time as was taken for each part of the picture to pass the aperture in condition (*a*) of Exp. IV). The subjects consisted of eight naval ratings with an average age of 19 and eight men and women of a comparable socio-economic level who were all in their seventies except one, who was 67 (average age 73).

The procedure and criteria for correct identification was as before, all eight pictures being exposed five times unless correctly identified sooner.

The results are shown in Fig. 5. The superiority of them to those obtained in the previous experiments is clear, especially among the older subjects.

Two other groups of similar ages and grades carried out the same experiment except that the exposure time was 3 sec., i.e. the time taken for the whole picture to pass the aperture at the basic band speed in the previous experiments (1 in. per sec.).

The results for these groups are shown at the right-hand side of Fig. 5. Correct identifications by the younger subjects are nearly complete on the first exposure. The older subjects during the first two exposures show results comparable with the tachistoscopic presentation at $\frac{3}{10}$ sec., but their correct identifications gradually climb to near the maximum possible by the fifth exposure, where the level they reach (i.e. after a cumulative exposure time of 15 sec.) is about the same as the younger reach at their first exposure at $\frac{3}{10}$ sec. Thus, although if they are given sufficient exposure time the older subjects reach the level of the younger, the extra time required is remarkably long.

Experiment VI. Varying aperture with exposure time constant

Further evidence of the importance of aperture as opposed to exposure time is furnished by an experiment in which aperture and band-speed were varied together so that the exposure time was kept constant at $\frac{3}{10}$ sec. whatever the aperture.

A group of eight naval ratings under 30 and a group of eight subjects from an Over Sixties Club (average age 69·9) were shown the same eight pictures under the following conditions:

(*a*) Four of the pictures with the first presentation at 0·1 in., the next at 0·2 in. and so on to 0·5 in. or correct identification.

(*b*) Four of the pictures with the first presentation at 0·5 in., the second at 0·4 in. and so on to 0·1 in. or correct identification.

The results are shown in Fig. 6 (*a*), and it can be seen clearly that the subjects do better when the wide apertures are presented first. The fact that the superiority of the wide apertures persists until the fifth exposure indicates that, as in Exp. IV, the wider apertures confer an initial advantage which persists.

The experiment was repeated with a further sixteen subjects of an occupational level similar to group 2 in Exp. II: eight with ages ranging from 25 to 38 (mean 29·8) and eight from 42 to 54 (mean 46·6). The results from this second group are shown in Fig. 6 (*b*) and were consistent with those of the former groups.

Repetition of responses

Some indication of one possible reason for incorrect identifications by older people is given by comparing successive replies. In the course of Exp. I there was evidence of a 'set' being formed by older subjects, the contents (or supposed contents) of one display influencing the response to the next. Seven of the older subjects showed this tendency, several of them more than once, and only three of the younger subjects did this, in no case as clearly. For example, an older subject gave this series of responses; to the arrow (Sil. I) 'shaped like a fan', followed immediately as a response to the star (Sil. I) by 'same as the fan but opens out like a tent' and then for the next stimulus, the triangle (Geom.

Figs.), 'a tent—a triangle'. The same subject, with the series at fixed aperture, gave these responses: having identified the yacht (Sil. II) correctly, responded to the monk (Sil. II) as 'someone signalling on a steamer', and to the next stimulus, a man carrying a pile of records (Pict. Rep.), 'something on the sea, a lighthouse'.

In a similar way, in this experiment, older subjects tended more frequently than younger to repeat errors made on one presentation of a picture when responding to a subsequent presentation of the same picture. The number of exact repetitions and the percentage they formed of the total errors are given in Table 6 (*a*) for all the experiments. It will be seen that a rise in repetition with age occurs often although not in all cases. In Exp. II, consideration was given to responses which, though not exact repetitions of previous ones, were very nearly so—the general content being the same and any changes being very small. Table 6 (*b*) shows that there was a consistent increase with age in such repetitions where the aperture was fixed at 0·5 in.

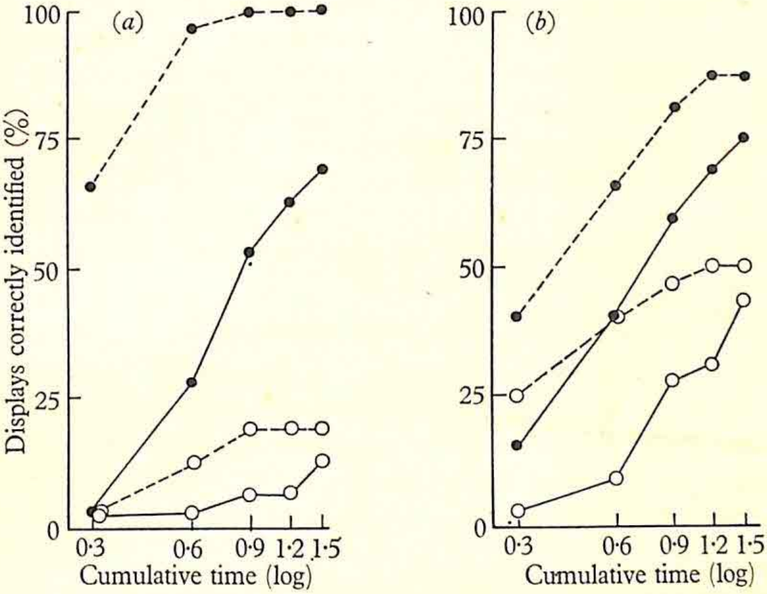


Fig. 6. Progress of correct identification with cumulative time, where exposure time was constant and aperture varied. Exp. VI: (*a*) group 1, oldest and youngest subjects; (*b*) group 2, intermediate age groups. *Key*: Younger subjects, closed circles; older subjects, open circles; decreasing aperture, dotted line; increasing aperture, black line.

Throughout the experiments, responses which contained material partly or wholly extraneous to the actual display were repeated more often than those correct but insufficient, but there was no consistent age difference here.

The tendencies to repetition are similar to those noted by Verville & Cameron (1946) in an experiment in which adults of different ages had to identify incomplete pictures, and in a learning experiment by Kay (1951). Verville & Cameron found that one picture was frequently wrongly interpreted as an animal, and older subjects perceiving the figure in this way persistently named animal after animal, instead of shifting to a different object when they found they were wrong. The fact that these tendencies appear among younger as well as older subjects in our experiments makes it seem probable not so much that they are a special feature of old age as that changes occur which may, in certain circumstances, throw them into increasing prominence as age advances.

Table 6. (*a*) *Exact repetition of responses, other than 'No Idea'*

	No. of repetitions	No. subjects repeating at least once	Av. repetitions per subject	Repetitions as percentage opportunity to repeat
Increasing aperture or speed				
Exp. I				
Under 30	17	10	1·7	13·0
Over 60	43	14	3·1	20·9
Exp. II				
Group 1				
21–30	3	3	1·0	5·7
31–40	15	6	2·5	20·3
41–50	5	4	1·3	6·9
51–60	11	5	2·2	11·5
61–70	13	6	2·2	13·3
71–80	12	5	2·4	10·5
Group 2				
21–30	3	2	1·5	4·6
31–40	5	4	1·3	7·5
41–50	1	1	1·0	1·6
51–60	4	4	1·0	3·9
61–70	8	6	1·3	8·6
Exp. IV				
Under 30	5	3	1·7	13·9
Over 60	18	6	3·0	16·1
Increasing aperture and speed				
Exp. VI				
Under 30	13	7	1·9	16·1
Group 2, av. 29·8	5	3	1·7	7·2
Group 2, av. 46·6	14	5	2·8	13·3
Over 60	15	8	1·9	12·3
Fixed aperture or speed				
Exp. I				
Under 30	15	11	1·4	15·6
Over 60	31	13	2·4	32·6
Exp. II				
Group 1:				
21–30	1	1	1·0	5·9
31–40	5	3	1·7	20·0
41–50	3	2	1·5	13·6
51–60	9	3	3·0	20·0
61–70	29	6	4·8	31·9
71–80	24	7	3·4	26·9
Group 2				
21–30	0	0	0	0
31–40	2	1	2·0	9·1
41–50	1	1	1·0	6·7
51–60	7	3	2·3	14·9
61–70	20	5	4·0	27·4
Exp. IV				
Under 30	8	3	2·7	30·8
Over 60	36	8	4·5	34·6
Decreasing aperture and speed				
Exp. VI				
Under 30	0	0	0	0
Group 2, av. 29·8	14	4	3·5	35·0
Group 2, av. 46·6	28	6	4·7	36·8
Over 60	26	8	3·2	23·4
Fixed aperture and speed				
Exp. V (tachistoscope)				
Under 30	2	1	2·0	22·2
Over 60	21	8	2·6	10·9

Note. 'Number of repetitions' alone is misleading because the better the performance the less the opportunity for repetition, but greater opportunity to repeat (because of incorrect identification) may be due partly to repetition. Thus, the first and last columns in this Table should be considered together.

Table 6 (*b*). *Experiment II. Exact plus very nearly exact repetitions of responses, other than 'No Idea'*

	No. of repetitions	No. subjects repeating at least once	Av. repetitions per subject	Repetitions as percentage opportunity to repeat
Increasing aperture				
Group 1				
21–30	10	5	2·0	18·9
31–40	28	6	4·7	37·8
41–50	16	5	3·2	22·2
51–60	30	7	4·3	31·2
61–70	20	8	2·5	20·4
Group 2				
21–30	7	5	1·4	10·8
31–40	13	4	3·2	19·4
41–50	6	4	1·5	9·5
51–60	21	8	2·6	20·8
61–70	20	6	3·3	21·5
Fixed aperture, 0·5 in.				
Group 1				
21–30	3	1	3·0	17·6
31–40	7	4	1·7	28·0
41–50	8	4	2·0	36·3
51–60	19	6	3·2	42·2
61–70	43	6	7·2	47·2
Group 2				
21–30	1	1	1·0	16·7
31–40	5	3	1·7	22·7
41–50	4	1	4·0	26·7
51–60	15	3	5·0	31·9
61–70	29	6	4·8	39·7

III. DISCUSSION

From the practical standpoint the indications of these experiments are fairly straightforward. Identification by older subjects of complex displays seen with only a part visible at once or for a restricted time, was much less accurate than it was by younger subjects. When the displays were simple the achievement of older subjects almost equalled that of younger. When they could be seen whole and for a longer time differences in performance with age became much smaller. Simplicity of display, full visibility and ample viewing time would, therefore, seem important conditions of success for older people.

Two sorts of differences have emerged therefore between older and younger subjects:

(1) *Where complexity of display varied*

It is suggested that, under conditions such as in Exp. I, the greater the complexity the greater the amount of temporarily meaningless material which has to be carried and integrated to produce correct identification. If it were simply the greater amount of material to be apprehended at once which was the source of difficulty older subjects should perform worse the wider the aperture, and worst of all under tachistoscopic conditions, and this does not happen. If temporal integration of *any* amount of material was equally difficult for older subjects their performance should not be worse with increasing complexity. It should be noted that meaningfulness of material apparently offset complexity for older subjects when complexity was not very great (Sil. II).

(2) *Where complexity was equal, but amount seen and length of viewing time varied*

Older subjects required either to see more of the display at once or a longer viewing time or both for their performances to equal those of younger subjects. Several possibilities can be suggested for this fact:

(*a*) *Directly physiological factors*, such as changes in the eye or in the central mechanism. The experimental results enable us to exclude one possibility under this head, namely, visual acuity as measured by test types with no account taken of reading time. Acuity in this sense of the term could not have been the cause of the observed age difference on performance because both young and old subjects were able to identify the pictures out of the apparatus and in the tachistoscope given sufficient time. Changes with age in the *speed* of dealing with fine detail in a display, as demonstrated by Weston (1949) may, however, be important, although how far these changes are dependent on peripheral factors and how far on central is difficult to say. The relation between acuity as normally measured with untimed tests and speed of perception and visual performance generally would seem to merit further study especially in relation to age.

(*b*) *Attitudinal factors*, such as extra caution on the part of older subjects. There is clear evidence of extra care being taken by older subjects in some sensori-motor tasks (see, for example, Brown in Welford *et al.* 1951), but it is not possible to say whether this is basically an attitudinal matter borne of experience, or whether it represents a shift in the manner of performance in the face of physiological changes in the organism which set a premium on economy of action and thus produce greater accuracy.

The increase with age in the present experiments of the number of indefinite and 'no idea' replies would be consistent with a theory that the older subjects were being more cautious, but would also be consistent with other theories, and cannot therefore be taken to support an explanation in attitudinal terms.

(*c*) *Experience.* Three possibilities need to be considered under this head:

(i) Experience increasing with age may widen the range of possible identifications by older subjects and thus increase the task of arriving at any particular one. Hick's (1952) law relating choice reaction time to degree of choice would lead us to expect that with increasing range of experience the time taken to make any particular identification would become longer.

(ii) Even if the total range of experience remains the same, some parts of it may become weighted at the expense of other parts as a man gets older. It seems reasonable to suggest that the process is akin to that demonstrated during these experiments in the influence of 'set' on perception. The experiments of Crossman (1953) and of Hymen (1953) would lead us to expect that objects to be identified which fall within the weighted part would be identified quickly, but those falling outside it would take a relatively much longer time.

(iii) The tendency for repetition of errors and the effects of 'set' shown in some of the present experiments and in those of Verville & Cameron (1946) suggests that effects of previous experience may have been operating on a very short time scale from one response to the next. It is possible that a subject's own previous response to a display may form a barrier to a change of interpretation. The greater repetition of responses involving extraneous material supports this suggestion. Kay (1955) has demonstrated such an influence with verbal material. There is some indication that such a tendency increases with age.

These explanations might also account for some of the increased slowness of older subjects with increased complexity.

In general the results bear out the expectations with which the experiments were started, in that they do show well-marked similarities between changes with age in perception and in sensori-motor performances, such as age differences with increasing complexity of material. Moreover, these age differences were, at least in part, due to slowness at reaching the same attainment rather than inability to attain the same level. Another similarity lies in the tendency of the older subjects to repeat errors even in the face of information that they were wrong (cf. Kay, 1951). These similarities do not, of course, prove that age changes in perception and sensori-motor performance are caused in the same way, but they do indicate that they share common functional principles.

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AN EXPERIMENTAL INQUIRY INTO THE CONCEPT OF PERCEPTUAL DEFENCE

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Experiments on the concept of perceptual defence can be considered as the result of two research interests: (1) attempts to relate experimental work to clinical findings and (2) attempts to determine the precise variables involved in the association between personality and perception. The concept of perceptual defence is based on the fact that subjects appear to show higher thresholds to taboo words and that autonomic responses to such words may be made before they are correctly recognized. This concept has been criticized on the grounds that the results may be more simply interpreted in terms of factors of word frequency and of suppression of responses by the subject. Other work has attempted to show that individual performance requires prime consideration.

This study has attempted to take account of these factors. A word-association test consisting of the most frequent words in the Thorndike and Lorge count was given to all subjects. On the basis of each subject's results, two lists were prepared for each subject, each list consisting of emotional and neutral words. A tachistoscopic presentation to the subject of one list was followed two weeks later by a presentation of the second list. It was found that six of the twelve subjects showed higher and two subjects lower thresholds to the emotional words on both occasions. The correlation between the two tests in terms of individual performance was highly significant.

Analysis of this and other experiments shows that the importance of factors of word frequency when familiar material is used remains to be determined. Further, some means of studying such factors other than by the use of word counts is necessary. It is suggested that a quantifiable 'hypothesis' or 'set' theory possesses the greatest advantages for an understanding of this area of research.

I. Introduction

The influence of past experience on perception has long been known in the study of human behaviour. The method by which such a process may operate has received several interpretations and awaits further clarification (see Bartlett, 1932; Oldfield, 1954). That the factors which affect perceptual processes may function without the conscious participation of the observer was known also to the earlier experimental psychologists (see Humphrey, 1951). Külpe (1904) and Jaensch (1930) further suggested that individual typologies may be related to characteristic forms of perception and imagery.

In keeping with the recent tendency to unify what were previously relatively separate fields of psychological study, attempts have been made to interpret current experiments on human perception in terms of theories of personality functioning, particularly that of Freud. These interpretations do not normally use the whole body of such theories, but attempt to apply certain specific principles, such as that of repression, to the experimental findings. This use of personality and psychodynamic theories has been especially congenial to those psychologists who have stressed the importance of internal, autistic factors as determinants of perception (Blake & Ramsey, 1951); in contrast, on the one hand, to those workers who place greater emphasis on the organizational properties of the perceptual field (Luchins, 1951), and on the other, to those who incorporate learning processes as the variables requiring prime consideration (Postman, 1953).

Stemming partly from Brunswik's functionalist interpretation of behaviour (Brunswik, 1955), a number of psychologists have attempted to examine the place of internal, or of that which they term 'behavioural', determinants in perception (Postman, Bruner & Walk, 1951). Certain of these psychologists, particularly Bruner (1951*a*, *b*), have attempted more comprehensive conceptualizations to account for the experimental findings and to relate such work to clinical observation.

In the course of experimentation with the latter theoretical orientation as a background, certain observations were determined which possessed a striking similarity to one of the suggested psychoanalytic mechanisms of defence. Such findings, however, have resulted in differences of opinion as to whether the phenomenon of perceptual defence actually exists. Further, if such behaviour can be reliably determined from controlled experimentation, can it not be interpreted in a more parsimonious fashion? This particular situation has resulted in the performance of numerous experiments with the purpose of clarifying the issues involved. Unfortunately, however, such a situation has made it extremely difficult for anyone who wishes to determine the present position to do so without a large degree of labour and confusion from the apparently contradictory findings.

It is the purpose of this paper to present the situation as concisely as possible, and, by means of an experiment which has attempted to take account of the several issues and experimental findings involved, to assist in some measure in bringing clarification into this important area of research.

II. EXPERIMENTAL DEVELOPMENT AND PRESENT POSITION

(i) *Initial establishment of the concept*

Perhaps the first occasion on which the term perceptual defence was used in this context was that on which Bruner & Postman (1947) suggested such a term as a partial explanation of the results of an experiment they had undertaken. To describe this experiment briefly: ninety-nine words, consisting of taboo words and neutral words, were presented to nineteen subjects as a word-association test. For each subject, the six presented words giving the longest response time, the six words of shortest response time and the six words of medium time were extracted. A fortnight later, each subject was shown these eighteen words by means of a tachistoscope, and the exposure times necessary for correct identification of the individual words were determined.

Seventeen of the nineteen subjects showed a significant curvilinear relationship between their individual reaction times to words and the exposure times necessary for correct recognition of these words. The results were then examined for the subjects as a whole. Recognition scores and reaction times were standardized around each subject's own means, and the relation between average recognition times and reaction times was given a graphical treatment. The resulting curve was of an inverted U form, suggesting to the authors that certain subjects showed higher exposure times for words which gave a longer reaction time and that other subjects recognized such words more easily and thus behaved in the opposite sense. To interpret these results, the authors presumed that individual subjects have differing selective forms of dealing with emotional words. Those subjects who required longer exposure times to words whose emotional significance was indicated by length of reaction time appeared to indicate the operation of a form of defence mechanism called perceptual defence. Those subjects who, on the other hand, saw such words more readily, indicate the operation of a form of perceptual sensitization to potentially disturbing words.

It was McGinnies's (1949) experiment which aroused the most discussion. In this experiment, words were not initially given in the form of a free word-association test, but a short list of eighteen words was prepared consisting of seven socially taboo words such as 'raped', 'bitch' and 'whore', with the addition of eleven neutral words. This list was then presented as in the second part of the previously considered experiment to individual subjects, and the tachistoscopic exposure times were determined for the neutral and emotional words. Records were also taken of the psychogalvanic responses of the subjects before and after recognition of each word. The results are essentially as follows: (*a*) taboo words require exposures of a longer duration for correct recognition than the words which are considered to be neutral; (*b*) psychogalvanic responses after words have been exposed and not yet correctly identified are greater when the words not reported as seen are taboo than when they are neutral. McGinnies interpreted these results to suggest that there was a mechanism by which visual stimuli were 'filtered' in order to protect the individual as much as possible from being aware of words which have an unpleasant emotional connotation.

(ii) *Word frequency and response suppression*

This experiment was soon subjected to a detailed criticism by Howes & Solomon (1950). The main points of their criticism were, first, that the duration thresholds for words vary as a function of the logarithms of the relative frequencies of usage of these words, as determined by the Thorndike & Lorge (1944) word frequency counts, and that the exposure times for recognition of both types of words would thus be of the same order when the effects of word frequency are extracted (see McGinnies, 1950). Secondly, that the experimental atmosphere, together with the presence of Prof. McGinnies and his female assistant, would lead to the withholding of responses to taboo words until the subject was absolutely certain that such words were being exposed. The possible feelings of a subject in such a situation are most amusingly described by Howes & Solomon, but their argument must none the less be taken as a serious criticism of the conditions of McGinnies's experiment. The subject could conceivably then be seeing a taboo word, with a consequent deflexion of the psychogalvanometer, before the word had been correctly recognized in terms of the experimenter's criterion of a correct verbal report.

Further papers by these two authors (Howes & Solomon, 1951; Solomon & Howes, 1951) tended to substantiate their previous findings and to suggest that word frequency was an equally important variable in the study of individual values as measured by the Allport–Vernon test. The second suggestion, however, was criticized by Brown & Adams (1954) and by Dukes (1955).

The experiment of Bruner & Postman (1947) was repeated in several respects by Delucia & Stagner (1954) but with allowances made for word frequency. Their findings were in agreement with the early suggestion made by Bruner to McGinnies (see McGinnies, 1949), that both the factors of frequency and the emotionality of words were involved in such experiments.

To examine the part played by the subject's attitude and by response suppression in these experimental situations, Postman, Bronson & Gropper (1953) used four main subgroups which were each given a different set of instructions prior to presentation of the emotional and neutral words. One group, as in McGinnies's experiment, were not informed that taboo words would be shown. A second group was clearly instructed to expect taboo words. A third group was not only warned to expect such words but was told that difficulties in recognizing taboo words was an indication of poor adjustment. A final subgroup was told that difficulties in perceiving taboo words was an indication of social adjustment. The resulting recognition thresholds were found to vary with the specific instructions given. The third group, for example, had the lowest thresholds for the taboo words and the first group showed the highest thresholds. It is significant that in this experiment, in which an attempt was made to control for word frequency, in the case of each subgroup, the thresholds for the taboo words were slightly lower than those for neutral words. The experimenters attempt to interpret this fact in terms of slight differences in frequency between the two sets of words as chosen for the experiment. Lacey, Lewinger & Adamson (1953), however, also noted that with an expectancy for taboo words, the threshold for such words was lower than for neutral words. Freeman (1954) and Bitterman & Kniffin (1953) also attempted to demonstrate that perceptual defence was not found when subjects were instructed to expect words of an emotional significance. The results of an experiment by Lawrence & Coles (1954) is interesting in that they showed that instructions were equally effective in reducing recognition thresholds when given either before or after the test flash. This finding would seem to indicate that it is the form of the subject's response which is affected by the conditions rather than his actual perceptions.

In two experiments carrying out substantially the same procedure (Solomon & Postman, 1952; King-Ellison & Jenkins, 1954), the frequency of syllables used was built into the actual experiment in order to study the relation between frequency and recognition thresholds. The subjects were initially presented with paralogs (pairs of nonsense syllables), certain of these paralogs being shown more often than others. A tachistoscopic presentation was then undertaken to determine the recognition thresholds for these paralogs. A high negative correlation was obtained between the frequency of initial presentation of the paralogs and their recognition thresholds, indicating that the less frequently a paralog was seen, the higher the threshold value necessary for correct identification.

(iii) *The clinical approach to the concept*

Two authors, Lazarus (1954) and Eriksen (1954), have undertaken a detailed criticism of the experimental work in this area. As these writers possess a similar point of view, it will be convenient to treat their opinions together. They agree with the objections of Solomon & Howes (1951) and of Postman *et al.* (1953) to the experiment of McGinnies, but feel that these authors have been rather selective in the experiments cited for criticism. In their opinion there are several experiments (to be treated shortly) which are not subject to such objection. Their own criticisms extend both to the McGinnies type of experiment and to that of Postman *et al.* (1953). These experiments, they consider, show two main defects. First, defence mechanisms from the point of view of Freudian theory may vary amongst individuals in their method of functioning. It is clear that there are other ways than repression of dealing with anxiety-arousing stimuli and that the treatment of data for groups as a whole would not show this. Secondly, if a form of perceptual defence is to be demonstrated, it is necessary to ensure that the words denoted as emotional are actually disturbing to the specific individuals. A particular word generally considered to be emotional may be in no wise disturbing to particular subjects.

Again, if frequency is a serious determinant of the phenomenon of perceptual defence, why is it that certain individuals recognize infrequent taboo words more readily? In the opinion of Lazarus, from an examination of the various experiments and of the data of Postman *et al.* (1953), word frequency is a serious factor only when highly infrequent words are used. From his point of view, word frequency may be considered equally well as a resultant of the personal needs and values of the whole population.

Lazarus and Eriksen cite experiments in which clinical typologies or the individual results of exposure to stresses have been found to correspond to the differential perception or recall of neutral and emotional words. Lazarus, Eriksen & Fonda (1951) demonstrated that good auditory perception of emotional material was characteristic of obsessive-compulsive patients. The reverse was found for hysterical personalities. Eriksen (1952) showed that subjects who had higher recognition thresholds and slower learning for words with a long delay than for words with a short delay in a word-association test, remembered more successes of a task. Those subjects who recalled more failures than successes learned words at a faster rate and had similar recognition thresholds for both short- and long-delay words in the association test. It is to be noted that in this experiment the subjects were instructed to expect possibly disturbing words. Finally, Lazarus & Longo (1953) showed that selective recall of failures in an interrupted task was associated with a relatively higher recall of nonsense syllables to which an electric shock had been paired. Those subjects who recalled a greater number of successes also recalled a larger number of syllables which had not been paired with the painful shock.

(iv) *Autonomic discrimination without awareness*

It has been mentioned above that in McGinnies's (1949) experiment, psychogalvanic responses were given to taboo words prior to verbal report. It was indicated that this may have been due to the possibility that the subjects were actually perceiving such words and yet not reporting them. Lazarus & McCleary (1951) undertook a most carefully controlled experiment to examine the possibility of discrimination without awareness. The complexity of design of this experiment does not allow a detailed report of their procedure and only the briefest description may be given. Ten nonsense syllables were presented separately to each of the subjects, five of these syllables being paired with an electric shock. When effective psychogalvanic responses had been conditioned to the shocked syllables, all ten syllables were presented at tachistoscopic exposure speeds too rapid for conscious discrimination (defined as the subject's inability to report which stimulus was presented). The syllables which had been paired with shock gave significantly greater psychogalvanic responses than the neutral syllables. Such autonomic discrimination without awareness is called 'subception' by the writers. As parts of this experiment have been misreported on occasion, it must be emphasized that there was no significant difference in the frequencies of identification of shocked and non-shocked syllables.

A number of writers consider that the subject obtains some information from the exposed syllable even though a correct verbal report is not given (Bricker & Chapanis, 1953). Howes (1954) has attempted a different account of discrimination without awareness by using a statistical theory which incorporates the subject's reports and which assumes the probability-like functioning of discrimination. Murdock (1954) has similarly subjected the interpretation of Lazarus & McCleary's experiment to some criticism.

(v) *Terminological aspects*

At this point the value of using a terminology derived from psychopathology must be touched upon. Klein (1954) objects to the immediate inclusion of psychoanalytic mechanisms as an explanation or description of the experimental results of such work. It is a presumption, he believes, to assume that if a form of defence is shown it must necessarily be the result of anxiety. The term 'defence' is useful when limited to forms of mastering anxiety, such as conflict among needs and the resulting avoidance of certain gratifications. Klein would prefer to use the term 'cognitive control' to indicate the particular observable form in which individuals manifest their internal processes in perception. It must be pointed out, however, that when the term 'perceptual defence' was first used by Bruner & Postman (1947), it was not intended to suggest that it necessarily corresponded to psychoanalytic processes but was used, as far as the present writer is aware, as a convenient descriptive term.

(vi) *Considerations for further experimentation*

The above broad treatment of the present position indicates that four factors in the design of such experiments require emphasis: (1) Factors of word frequency must be effectively controlled. (2) The data and the design of the experiment must be such that individual performance can be examined. (3) The words used must be of emotional significance to the individual subjects in the experiment. (4) The experimental conditions must be such that the subjects will not suppress their responses.

It has been the purpose of the writer of this paper to include these considerations in the design of the experiment to be presented in the following section.

III. EXPERIMENT

(i) *Selection of materials*

The essential difficulty in allowing for factors of word frequency was to obtain a frequency count which could be used satisfactorily with subjects who had spent their lives in Britain. Extensive inquiries failed to elicit the existence of such a frequency count of any comprehensiveness. The word counts available for use in this country, if not based upon American sources, were of such a nature that, although they have proved of value for language teaching, the wide coverage of source material essential for precise experimentation was lacking. Thus the only comprehensive source that could be used, if the experiment was to replicate to any extent the previous work in this field, was the Thorndike and Lorge *Word Book* (1944). It was decided to use the thousand most frequent words in the American language as indicated in this word count. The advantages of this particular selection appeared to be several. First, the sources from which these most frequent words were taken consisted of a relatively greater proportion of common British classical and children's books of the type with which the subjects are most likely to be acquainted. Secondly, the use of the most frequent words would meet the suggestion that frequency factors in relation to recognition thresholds are negligible when the words used are very common. The value of this point rests on the possibility that the most frequent American words in the count are not too dissimilar to the most frequent British words. Thirdly, the words used were of equivalent frequency and possible deviations from this range are likely to be minimal. Fourthly, it was considered more valuable to include words of known equivalent frequency than to use either the methods which are often employed or to use those methods which, by the inclusion of certain taboo words of no reasonably known frequency, introduce an unnecessary degree of arbitrariness into the interpretation of the experimental findings. Finally, the use of words of such a common character greatly minimizes the possibility of the subjects suppressing their responses as a result of the experimental conditions.

In order thus to prepare a list of words for use in a word-association test and for later tachistoscopic presentation, the total number of five-lettered words (two hundred and four) were extracted from the thousand most frequent words in the Thorndike and Lorge word count. These words were then given separately to five male members of the department, who were asked to choose the fifty words from amongst the total number which they considered to be the most emotional in connotation, and the fifty words considered to be the most neutral. A final list was compiled consisting of forty-five emotional words and forty-five neutral words in the choice of which four of the five judges had agreed. This list was arranged in a random order and is shown in Table 1.

Table 1. *List of forty-five 'emotional' and forty-five 'neutral' five-lettered words used in the word-association test**

about	since	north	*class*	*mount*
board	these	*under*	bring	reply
laugh	fruit	raise	built	going
enter	*alone*	sweet	*large*	*voice*
women	*small*	*young*	earth	*close*
blood	month	*storm*	*fight*	*first*
until	*court*	whole	those	*teach*
learn	*happy*	*truth*	white	clear
would	chair	guess	*sleep*	right
seven	prove	apple	*smoke*	*spend*
child	*marry*	south	among	paint
place	quite	still	along	*scene*
break	*death*	*dress*	*enemy*	sense
eight	*trust*	offer	heavy	*uncle*
cross	*money*	*dream*	*night*	ready
hurry	music	*shout*	*mouth*	*think*
tried	reach	*study*	*guard*	light
there	*power*	*drink*	third	today

* The words selected as emotional are in italics.

(ii) *Word-association test*

The subjects were twelve in number, all of whom were female, and consisted of four psychology undergraduates and eight other undergraduates or girls of the administrative staff. All the subjects were acquainted and on friendly terms with the experimenter. None of the subjects was informed as to the purpose of the experiment. On questioning the subjects when the full experiment was completed, it was determined that none of the subjects possessed any idea of its real purpose. When initially asked to participate in the experiment, they were told that the aim of the experiment was a general investigation into the 'seeing' of words. In the course of the questioning, two of the psychology undergraduates professed a minor acquaintance with the experiments of McGinnies (1949) when mentioned, but were vague as to its findings and did not at all relate it to the present experiment. These two subjects gave results contrary to each other when acting as subjects in the experiment.

The prepared list of ninety words was presented on separate occasions to each of the twelve subjects as a free word-association test. The following instructions were read to the subject at the commencement of the test:

> I will call out a word and I would like you to reply with the first word which comes into your mind. It needn't be connected with the word I am giving; I just want you to say as quickly as you can the first word which pops into your head. I am not going to write down your replies. All that this clock does is to tell me the time it takes, which is all in which I am interested. The word given will only be said once. I will try to say it as clearly as I can, so please don't ask me to repeat it.

An electronic 'Labgear' timer (Type D 4102) using Dekatron glow-transfer tubes was started when the experimenter (male) called out a word and simultaneously pressed a key. A voice-key was used such that the subject's response stopped the activity of the electronic timer, indicating the response time to a hundredth of a second. This first procedure required an average time of 25 min.

Two lists of words were prepared for each subject from the subject's response times for use in the two subsequent tests. The sixteen emotional test words to which the subject had given the longest response times and the sixteen neutral words to which the shortest response times had been given, were extracted from the complete list of ninety words. Two lists were then prepared, each consisting of eight of the emotional words and of eight of the neutral words, arranged in a random order. Words were apportioned to each list so that the mean response time to the emotional words in one list was approximately equal to the mean response time to the emotional words in the second list and similarly for the neutral words.

(iii) *First tachistoscopic presentation*

This test took place at a period of approximately a fortnight after the initial word-association procedure. Ninety cards were prepared for use with the tachistoscope. On each card, white in colour, one of the ninety initial test words was stencilled in black Indian ink in letters of $\frac{3}{16}$ in.

The tachistoscope used consisted essentially of a frame into which the card was inserted. In front of the frame were two gas-filled bulbs, the light from which was reflected by the card and focused by means of an anastigmatic camera lens with a variable speed shutter on to a ground-glass screen 4 by 5 in. in dimension. In front of the screen, which was hooded, was stretched a length of thin white cord. A black mark at the middle of the cord indicated that the projected word would be seen a quarter of an inch above the mark when the observer was correctly seated.

The subject was seated before the tachistoscope and the following instructions were read by the experimenter:

> This is an experiment in seeing or perception. I am going to flash a five-lettered word for a fraction of a second on to this screen. The words will look like this (a sample card is shown). I want you to tell me what the word is when it appears on the screen just above the black mark. Before each time I flash a word on to the screen, I'll say 'ready', and two seconds later it will appear. It is quite likely that you won't be able to recognize the word the first time or first few times, but I want you to tell me what it is as soon as you can make it out. I would like you to tell me when you see even only a part of the word. It's all right to guess. Is there anything which you are not sure about? All right! We will adjust your position and then we will do two practice words to show you how easy it is. After that we will go straight on to the others.

The seating of the subject was immediately adjusted so that a sample word projected with a high intensity of illumination and of a long duration could be seen clearly and at a height of $\frac{1}{4}$ in. above the black mark. The average intensity of illumination of the adapting field was 1·72 apparent foot-candles and of the test field 1·75 apparent foot-candles. Two practice words (one emotional and one neutral) were then shown in accordance with the following procedure: the card was inserted into the frame by the experimenter, who was hidden by the tachistoscope, and the intensity of illumination of the bulbs (indicated by a light-meter inside the apparatus) was adjusted with the aid of a 'variac' resistance to 1 foot-candle. The word 'ready' was called and the test word was flashed on to the screen 2 sec. later. All words were to be projected for a constant duration of $\frac{1}{25}$ sec. The practice word was projected in increasing steps of 1 foot-candle of intensity of illumination until the subject gave two consecutive correct verbal reports. This procedure was repeated for the second sample word. Following the practice words, the sixteen test words were shown with the same procedure. These words, however, were projected at an initial intensity of illumination of 10 foot-candles below the lowest value at which the subject had correctly recognized the practice words.

The exposure time of $\frac{1}{25}$ sec. was used to ensure that projected words would be exposed for a period below the critical duration time in order that the Bunsen–Roscoe law would hold and that the product of intensity of illumination and of exposure time could thus be assumed to be constant (see Woodworth & Schlosberg, 1954). With reference to the fore-period of 2 sec. used in the experiment, it is appreciated that this value, as determined by Woodrow (1914), cannot now be accepted without question. On the basis of the writer's previous experience, however, and in the light of the observations of Teichner (1954), it was decided to retain such a fore-period in the experiment.

(iv) *Second tachistoscopic presentation*

A fortnight after the first presentation to each subject of her sixteen test words, the second list of sixteen test words was given. The experiment took place under exactly similar conditions. As before, the subject was seated in front of the tachistoscope. The previous adjustments and instructions were repeated, and with the exception of the use of new practice words, the experiment was continued in exact accordance with the above procedure.

(v) *Results*

To determine whether there was a significant difference between the reaction times to the forty-five emotional words and the reaction times to the forty-five neutral words, the Mann–Whitney test was applied to the results of each subject. Only subject 2 (see Table 2) showed a significant difference ($P < 0·05$). However, it can be seen from the table that, with the exception of subjects 3 and 5, all the subjects showed a slightly longer mean reaction time to the emotional words. The Wilcoxon matched pairs test (Mosteller & Bush, 1954) was therefore used to ascertain whether the reaction

Table 2. *Mean reaction time of each subject to each of the two classes of words*

	Subject											
	1	2	3	4	5	6	7	8	9	10	11	12
Emotional words	1·27	1·54	1·47	1·07	2·26	2·06	1·73	2·65	1·52	1·71	2·41	1·85
Neutral words	1·19	1·19	1·49	1·04	2·33	1·95	1·66	2·57	1·38	1·58	2·19	1·75

times of the group as a whole to each of the two classes of words were significantly different from each other. A significant difference was found ($P < 0{\cdot}01$; smaller sum of ranks $= 5{\cdot}5$).

From each of the two tachistoscopic tests, two mean recognition thresholds were obtained for each subject (see Table 3); the mean recognition threshold for the eight emotional words shown ($E1$ and $E2$) and the mean recognition threshold for the eight neutral words shown ($N1$ and $N2$). The differences $E1-N1$ (in the case of the first test) and $E2-N2$ (in the case of the second test), indicate the amount by which the mean recognition threshold for emotional words exceeds the mean recognition threshold value for neutral words. A positive value for this difference would indicate that the mean recognition threshold for emotional words was greater than the mean recognition threshold for neutral words. A negative value for this difference would indicate the reverse.

Table 3. *Individual results in the first and second tachistoscopic tests*

	First tachistoscopic test			Second tachistoscopic test		
	Mean recognition thresholds (ft.-candles)*			Mean recognition thresholds (ft.-candles)*		
Subject	Emotional words ($E1$)	Neutral words ($N1$)	Difference ($E1-N1$)	Emotional words ($E2$)	Neutral words ($N2$)	Difference ($E2-N2$)
1	18·63	16·00	2·63	23·63	21·50	2·13
2	20·63	20·00	0·63	16·13	14·13	2·00
3	15·00	18·13	−3·13	20·38	22·75	−2·37
4	13·50	14·75	−1·25	12·38	13·88	−1·50
5	18·63	18·00	0·63	12·63	12·00	0·63
6	21·88	21·56	0·32	17·38	17·50	−0·12
7	13·88	12·38	1·50	14·63	13·50	1·13
8	16·88	16·56	0·32	11·75	12·00	−0·25
9	13·38	14·00	−0·62	14·63	14·50	0·13
10	15·00	12·38	2·62	21·00	18·00	3·00
11	16·63	15·13	1·50	15·63	15·75	−0·12
12	16·13	14·50	1·63	17·00	15·63	1·37

* Recognition thresholds represent in each case the mean recognition threshold value of eight words.

The results of this experiment would allow the possible application of at least three forms of treatment. (1) For each test, it can be determined whether the mean values of columns $E1$ and $N1$ or $E2$ and $N2$ are significantly different from each other. (2) It can be similarly determined, for each subject, whether the mean recognition threshold for the eight emotional words is significantly different from the mean recognition threshold for the eight neutral words. (3) The agreement—in both extent and direction—between the performance of subjects in the first tachistoscopic test and their performance in the second test may be computed. Procedures (1) and (2) would allow application to either a single test or to the results of both tests. It was initially decided, on the basis of the observations in § II (vi), that individual performance in both tests could be most fruitfully examined by using the third treatment. The use of procedure (1) would consist of treating the results as a whole. The differing performance of individual subjects would obscure the opposite performance of other subjects and would leave an insignificant value, although certain subjects behaved consistently in one direction. Procedure (2) could not be used satisfactorily, as the initial selection of such common words—differing to such a slight extent in emotional connotation—would place an undue strain on the possible contribution of factors of emotional selectivity as measured by this type of experiment. Treatments (1) and (2) were applied, however, to the results of both the single tests and to the results as a whole. No significant differences were found with the use of either non-parametric statistics or transformed data. The third treatment was therefore carried out. The correlation between the two difference columns $E1-N1$ and $E2-N2$ was computed by means of Kendall's rank correlation method (Kendall, 1948). This correlation was found to be highly significant ($\tau = +0{\cdot}73$, S.E. $=0{\cdot}22$). The value obtained using Spearman's coefficient was $+0{\cdot}85$ ($P<0{\cdot}001$, $t=5{\cdot}07$, D.F. $=10$).

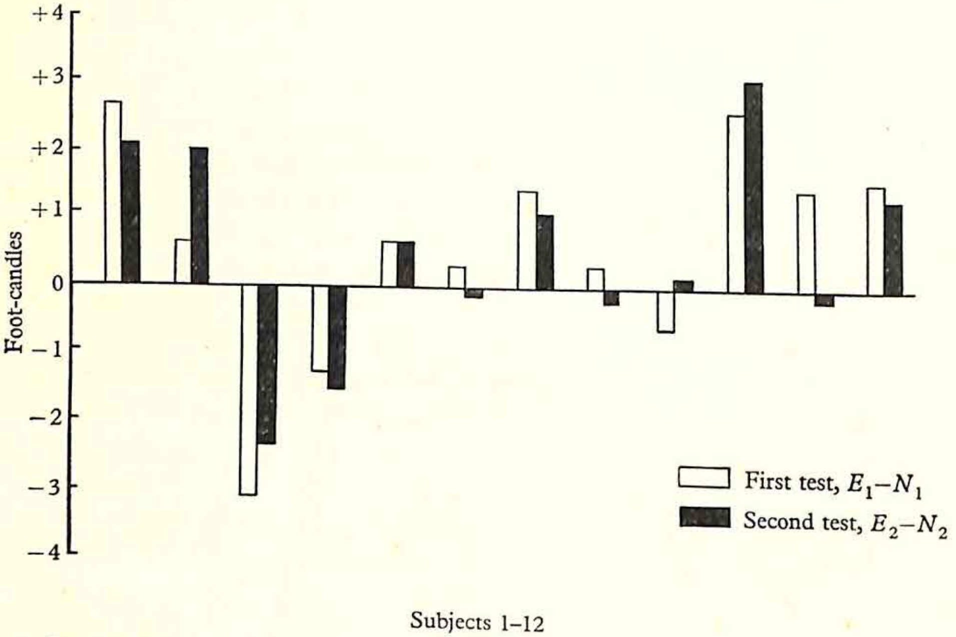

Fig. 1. Differences between mean threshold values for emotional and neutral words of individual subjects in both tests.

Fig. 1 shows the performance of each subject in the first and second tachistoscopic tests. The length of a column indicates the difference between mean threshold values for emotional and neutral words and thus corresponds to the value of either $E1-N1$ or $E2-N2$, as shown in Table 3. A column above the horizontal axis indicates that this difference was positive. Each pair of columns represents the results of one subject in the first and second tests. The ordering of columns corresponds to the position of the subjects in Tables 2 and 3.

It can be seen from an inspection of Fig. 1 that the subjects showed the characteristic perceptual defence and sensitization in relation to emotional words referred to in our survey. Six of the subjects repeated their performance of possessing higher thresholds for emotional than for neutral words. Two of the subjects showed lower thresholds for emotional than for neutral words on the two occasions.

IV. Discussion

In this experiment, and in several others, the objective criterion of the emotional connotation of a word has been the duration of reaction time. Strictly speaking, the present experiment has only shown that higher thresholds (and the reverse) are obtained for words of a long reaction time; that the words were actually emotional has not been unequivocally demonstrated. Our present knowledge indicates that two determinants are mainly responsible for the duration of reaction time: the associative strength of the stimulus word and its emotional effect on the subject. The more recent work on this question appears to indicate that with normal subjects it is the associative strength of the stimulus word which is the greater determinant of the length of reaction time.

It was shown earlier that the mean reaction times of the group as a whole to each of the two classes of words were significantly different. Our extraction, in the case of each subject, of the emotional words giving long reaction times and of neutral words of short reaction times may conceivably have resulted in words which differ mainly in their associative strength. We cannot assume that the use of a word count effectively controls individual differences in factors of word frequency. Laffal (1955), using words of roughly equivalent frequency from the Thorndike and Lorge count, has found a high correlation between long reaction times to the stimulus words and the number of different responses made to these words by the group as a whole. We should therefore expect, on the basis of Solomon & Postman's (1952) experiment, that words giving a longer reaction time would require higher recognition thresholds. Such an interpretation does not satisfactorily explain the performance of those subjects who show a greater sensitivity to stimulus words which earlier gave a longer reaction time.

The writer wishes it to be noted that all the studies which show the importance of the associative strength of words have been carried out with groups of subjects. That it may not be learning factors alone which determine recognition thresholds or length of reaction time is indicated by the fact that the correlations found do not show complete agreement. This is shown even when information measurement is applied to the data. Certain of the subjects, therefore, may not be showing such relationships. Further, the influence of emotion or the law of effect, whatever theory is held about their mode of action, must clearly have a place in determining the associative strength of words measured in frequency experiments.

How may we interpret, then, the findings of this and related experiments? The measurement of psychogalvanic responses, not only during the tachistoscopic session, but also during the word-association test, would be a valuable adjunct to an interpretation of the mechanisms involved. Again, the experiments which appear to show the persistence of individual forms of 'cognitive control' in varying situations, or which show a correspondence between clinical typologies and the differential perception of emotional words are suggestive. However, in this type of experiment, much requires to be understood regarding the processes which are responsible for these characteristic forms of perception.

Perhaps one of the most valuable ways of understanding our findings lies in the 'hypothesis' theory of Bruner (1951*a*) and his collaborator, Postman. Without entering into a detailed presentation of this particular theory, we can examine its application in a simplified form to a naïve laboratory subject. When the subject is seated before the tachistoscope and is informed that five-lettered words are to be shown, we can consider that she possesses a particular hypothesis or expectancy that common words will be presented. When, as in the preliminary stages of presentation, the letters are lacking in clarity and several different interpretations are possible, the strong hypothesis that common words are being shown is relatively easily confirmed. The thresholds for common words will therefore be lower. If, on the other hand, the subject holds the strong hypothesis that the experimenter will actually show uncommon or taboo words, we can expect that these words will be more easily recognized.

This type of theory, with its various refinements, works well when the words are sharply distinguishable in character, as in the usual type of experiment. In the present experiment, however, the differences between emotional and neutral words are very small indeed. Bruner's theory is to be considered as a conceptual model, and thus can also be applied to our findings. When the term 'hypothesis' or 'expectancy' is used synonymously with the word 'set' and a definite physiological basis is presumed for these processes, as in Allport's (1955) interpretation, we find certain difficulties. In other words, can a theory which presumes a physiological basis for 'hypotheses' account for the different thresholds for words which differ to such a slight extent in nature?

Certain considerations are suggested by the results of the present study. There is a need to ascertain just what place factors of frequency have in determining perceptual thresholds when the material used is highly common. There is a need to find some method of studying such factors which is independent of the use of frequency counts. Again, one must agree with Vernon (1955) that one of the greatest defects of this form of research is its undue concentration on artificial situations. The use of concepts such as 'hypothesis' or 'set' has the greatest advantages for an understanding of the present type of experiment; not only because it gives a possible measurable basis to the processes involved, but because it suggests a means of combining the large amount of American work in this field with the peculiarly British contribution in the field of schemata.

The writer wishes to acknowledge the valuable assistance received from Mrs Pearl Segal, Miss Thelma Veness, Mr J. Brown and Mr N. Brown. He particularly wishes to express his gratitude to Mr B. M. Foss for his advice and criticism throughout the experiment and to Mr E. Wasservogel for his careful construction of the apparatus.

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PUBLICATIONS RECENTLY RECEIVED

The Child's Conception of Space. By JEAN PIAGET and BARBEL INHELDER. Translated from the French by F. J. LANGDON and J. L. LUNZER. London: Routledge and Kegan Paul. 1956. Pp. xii + 490. 42*s*.

It is greatly to the credit of British psychologists and publishers that so many of the major works of Jean Piaget have first appeared in translation in this country, rather than the U.S.A. Mr Langdon and Mr Lunzer must be congratulated on the excellence of their translation of the often difficult and abstruse argument of this book. It is to be hoped that due recognition may now be given to the outstanding contribution made by Piaget and his collaborators to our understanding of the nature of the thought processes as they develop from infancy through childhood to the adult stage.

The Child's Construction of Reality described the investigation of the infant's immediate perception and his early understanding of the nature of objects. *The Child's Conception of Space* (first published in 1948 as *La Représentation de l'Espace chez l'Enfant*) proceeds to demonstrate how this stage is succeeded at a considerably later age, first by an intuitive understanding of the manner in which objects are related to one another in space. This is followed by a pre-logical grasp of spatial relations within projective space to the viewpoint of the observer; and finally by completely logical concepts of integration within the co-ordinates of Euclidean space. The child works out the successive stages for himself by means of appropriate motor activity, the images of which are integrated within ideational schemata, leading finally to what Piaget calls 'operations'.

Though they may find the treatment by Piaget and Inhelder of this development in terms of mathematical logic difficult to comprehend, all psychologists will welcome the ingenious experimental demonstration of the manner in which the various spatial concepts evolve. Thus it is shown that the earliest notion of spatial relations is the topological, since this constitutes the simplest form of spatial organization. Experiments on tactile perception of shape and on drawing simple and complex forms demonstrate that the understanding of proximity and separation precedes that of simple geometrical shape, especially where angles and inclined lines are involved. Experiments on threading rows of beads show the grasp of order; and on the tying of knots, that of surrounding or enclosure. The concept of continuity, which draws together these notions, appears in the synthesis of points to make a continuous line.

The development of ideas about projective space is then demonstrated in the correct judgement of parallel lines and of the shadows and cross-sections of geometrical solids. But, most interesting of all, is the ability to envisage the appearance of landscape scenes from different points of view. For it is only when the child has grasped the effect of difference in viewpoint that he begins to understand how the disposition of objects in space may be related to the position of his own body; and hence the notion of projective space in general. But the final stage, the understanding of Euclidean space, comes only when the measured distances of objects from each other and from the observer can be related within the framework of a universal system of co-ordinates. This is demonstrated by two most important experiments. The first, on estimating the position of the surface of a liquid in a tilted vessel, shows that the child does not reach an immediate understanding of the horizontal and vertical co-ordinates of space from his direct perception of the horizon and of vertical objects, but develops it gradually up to about the age of 9 years. The second, on the reproduction in a scale plan of the positions of scattered objects, demonstrates that the total co-ordination of spatial positions within a single whole is not attained till the age of 10–11 years.

Piaget's experimental techniques have been criticized for their lack of experimental control. In fact, the descriptions of the behaviour of children and their answers to questions in these experimental situations do not prove that all children proceed through these stages of development; nor can exact age norms be derived from them. Further experiment with larger numbers of children is needed; or, even better, a longitudinal study in which the same children are followed up at successive ages. But these observations, even as far as they go, add enormously to our understanding of the conceptual development of children; and of the extreme complexity of spatial concepts and ideas which to the adult seem so simple and obvious. It is impossible in the space of a review to demonstrate satisfactorily the subtlety and profundity of Piaget's argument. Thus it is to be hoped that all psychologists will study it for themselves.

M. D. VERNON

Risk and Gambling: the Study of Subjective Probability. By JOHN COHEN and MARK HANSEL. London: Longmans, Green. 1956. Pp. x+153. 14*s.*

In recent years guessing behaviour has become a phenomenon of interest to psychologists. In part this interest has been aroused by a concern for psychophysical methodology, and in part also by an increased awareness of the problems involved in the study of human thought processes. This book is a very timely addition to the literature available. It describes a series of experiments of a guessing or gambling nature, carried out by the authors mainly on children. Thus it has additional value in improving our knowledge of children's concepts of causation. The experiments are extremely ingenious, and will undoubtedly be repeated as class demonstrations in many university courses and other lectures in general psychology. Psychologists and statisticians who are exercised about problems of response dependencies in those situations demanding repeated observations from the same individual will find their fears confirmed, and be driven further to elucidate 'psychological randomness' and to devise procedures to minimize its unwanted effects.

It seems churlish to point out defects in the book, but, as it is the first publication on these problems to reach the non-specialist public, some comments must be made. Excellent though the experiments are, there are many others, mainly in the American literature, which could well have been cited in support. A few of the earlier post-war references are given in footnotes. Further, there is no reference to the nineteenth-century statistical literature on the relation between probability and belief (cf. especially Venn) which is so relevant. Perhaps most surprisingly of all in work which is primarily an exercise in the psychological significance of Bayes's Postulate, Bayes is never mentioned. But with those defects (which are to some extent offset by due appreciation of Piaget's approach), this work remains a valuable and important contribution to one of the central problems in psychology.

J. W. WHITFIELD

The Normal Child and some of his Abnormalities. By C. W. VALENTINE. London: Penguin. 1956. Pp. 291. 3*s.* 6*d.*

Though the subtitle of his book is 'A General Introduction to the Psychology of Childhood', Prof. Valentine has set himself the very specific task of considering what Prof. Mace in his foreword describes as 'a question of great concern to parents and teachers to-day', namely, 'Is this child "normal" or shall I take him to the clinic?' In the course of offering a basis on which this question may be answered, Prof. Valentine considers children of all ages from infancy to early adolescence and in a wide variety of activities and human relationships. In spite of this, it is doubtful whether his central question can provide enough integration wholly to justify the subtitle. The book certainly cannot be called 'popular' in any pejorative sense; as one would expect, it is written with scrupulous concern for the citing of evidence and the weighing of arguments. But, as the author says himself, it 'only seeks to be descriptive and explanatory'. It may reasonably be argued that a general introduction should be more largely concerned with general theory than this book, given its first and guiding aim, is able to be.

However, as a source of reassurance to the anxious parent, troubled by scraps of psychological information—or misinformation—and by vague notions about 'neurosis', its value should be considerable. It offers for such a parent the opportunity to develop perspective and to acquire standards of comparison or, to use the word suggested by the title, 'norms'. One of the difficulties in any attempt to do this is, of course, this very word 'normal', with its varying and conflicting uses and connotations. Prof. Valentine begins his book with a direct attack on the question of what may be meant by the 'normal child'. 'The word "norm"', he says, 'means a rule, standard or average.' The trouble, however, is that the first and second of these words share all the ambiguities of 'norm' itself. The 'standard' may be the average or it may be the ideal to be aimed at, in the sense in which standards are set. This distinction between the statistical and the evaluative 'norm' is a common source of confusion, since an approach to the standard in one sense is generally a departure from the standard in the other. And a corresponding but perhaps even greater difficulty is caused by the word 'abnormal', for the negative introduces the problem of deciding at what point of remoteness from the statistical or from the evaluative 'norm' behaviour is to be considered statistically or evaluatively abnormal. It might have been helpful if Prof. Valentine had considered these issues more fully than he does. He may well have felt that any very detailed discussion would be out of place in a book of this kind, but, in its absence, there must be a certain risk of recurring perplexity for any reader who does not come to the work with a prior awareness of the main distinction.

MARGARET C. DONALDSON

Psychology: the Fundamentals of Human Adjustment. (Third edition.) By NORMAN L. MUNN. Boston: Houghton Mifflin Company. 1956. Pp. xvi + 542. $5.75.

Dr Munn has reorganized his popular text-book and brought it thoroughly up to date. Unexpectedly this has led to a reduction in size effected by widespread pruning and by giving up the former arrangement of chapters in divisions each with its own introduction. The twenty-four chapters of the second edition have been reduced to sixteen rather longer ones without any noticeable contraction of coverage. In some directions there has been an extension to cover formerly neglected topics, such as fatigue.

A new sequence of topics has been adopted in order to shift neurology to the end of the book and to '...capitalize, at the outset, upon the interest of students in individual differences...'. The new chapters in turn deal with *The Scope and Methods of Psychology, Individual and Group Differences, Intelligence, The Motivation of Behavior, Emotional Motivation, Frustration and Conflict, Personality, The Learning Process, Foundations of Learning, Remembering and Forgetting, Thinking, Attending and Perceiving, Vision, Hearing and the Other Senses, Neural Foundations of Behavior*, and finally, *Working Efficiently*. Difficulties raised by this sequence are most obvious in the discussion of emotion where the Bard–Cannon theory precedes any mention of neurones and conditioning appears prematurely in connexion with John Watson's theory. Most of the chapters are only loosely integrated. The order in which they are set down need not dictate the order of a course based on the book.

Since more than one-sixth of the references and more than half of the suggestions for further reading date from 1950 or later, this edition is well up-to-date. It is to be regretted that psychological writings should become obsolete as quickly as they do.

The 35-page glossary is a useful new feature but would be improved by pruning non-technical words, such as *rage, reproof, research, rivalry, dreaming* and *love*. The last is defined non-technically as 'A feeling of attachment or affection...not necessarily sexual', but the only indexed reference is to the word as used by John Watson. Some of the definitions of important terms are weak. Emotion is defined as 'A condition underlying such experiences and actions as occur in fear, rage and the other so-called emotions'.

Generally the text is carefully written, but now and again loose expressions occur. We are told on p. 243 that statistical analysis of some experimentally observed differences shows that 'the chances are almost 100 in 100 that they result from different motivation, and not from chance factors'. A picture of a distorting room bears the caption 'How our eyes can deceive us'. Giving examples of constancy Munn writes '...the square table top is continually seen to be the same square...' in spite of changes in the retinal image.

The book sets out to attract students. Illustrations are so plentiful that the chances of opening it and being confronted with two pages of unbroken type are about 3 to 1 against. The judicious banishment of calculational procedures in statistics to an appendix has further reduced the chance of a student's encountering aversive stimulation. Most of the illustrations are well chosen, but a few, e.g. a comic strip of Poincaré going through the four stages of creative thinking, are only distracting and may embarrass the serious student.

The text preserves the same flavour as earlier editions despite some shift in emphasis from rats to people and from the physiological to the social aspects of the basic processes. Experimental psychology and the S.–R. approach are most fully represented, but Dr Munn is able to take up a wide variety of points of view with sympathy. At times there is an irritating nebulousness about the discussions of basic processes, but this is an almost inevitable result of eclecticism. Admirers of the earlier editions will not be disappointed with the changes that have been made. In many ways the book remains the best of its type.

Some degree of colour shock may be induced by the new jacket.

R. L. REID

A Primer of Freudian Psychology. By CALVIN S. HALL. London: Allen and Unwin. 1956. 13*s.* 6*d.* (British edition; first United States edition published in 1954.) Pp. xii + 137.

The first thing that must be said about this book is that it would be difficult to improve on its workmanship—I might almost say 'craftsmanship'. Prof. Hall has set himself the task of giving an account in Freudian terms of the development of normal personality, as opposed to abnormalities. Most attempts of this kind are either superficial, short and implausible or thorough, technical and long; but this one manages to be short, plausible and yet technically accurate. The secret of this achievement seems to lie partly in the systematic order that Prof. Hall has worked out for introducing the various interlocking concepts without repetition or tiresome cross-references. But it also lies in his knack of graphic yet economical explanation. Here is the Freudian theory of a frequent cause of neurotic anxiety, epitomized in eighteen words: 'The id says "I want it"; the superego says "How horrible"; and the ego says "I am afraid".' Could anything be shorter or clearer?

Nevertheless, the author is very careful to include only what can be found in Freud's published works, and to exclude early views which he can be shown to have abandoned. This is so conscientiously done that I was proud to be able to find a few doubtful passages, which I dare not do more than question. Did Freud really believe that 'There would be no psychological development if every time the baby began to feel the tension of hunger it was immediately fed and if all of the other excitations arising in the body were similarly discharged by the co-operative efforts of parental care and inborn reflexes'? Is Freud responsible for the over-simplified physiological account of asthma on p. 89? Is 'predicate-thinking' really a Freudian term? And does not the 'primary process' include condensation and displacement as well as the production of 'a memory-image of an object that is needed to reduce a tension' as Freud implies, for example, on p. 52 of his *Outline of Psychoanalysis* (1940)?

These are minor details, however. I have a more serious doubt about the conception of this book. Some of the Freudian theories which it outlines have been considerably modified by Freud's own followers, such as Klein and Fromm (unless one defines 'follower' as 'one who makes no modifications'). It would have been useful if Prof. Hall had outlined these changes as clearly as he has dealt with the original. The alternative was to recognize that an exposition of Freud and nothing but Freud is of historical interest, and to treat it historically by tracing the development of his theories (a task which Dr Ernest Jones has not rendered unnecessary). But—probably for the sake of simplicity—Prof. Hall has tried to make his book a *cento* of Freud's last thoughts, and has thus reduced its usefulness. The book is otherwise so good that this is doubly unfortunate, and I hope that the second edition will contain at least footnotes to indicate the main neo-Freudian developments.

NIGEL WALKER

Freud and Religious Belief. By H. L. PHILP. London: Rockliff. 1956. Pp. xi+140. 18*s*.

Not unlike Shelley, Swinburne, Nietzsche and Charles Bradlaugh, Freud, despite his genius for interpreting the activities of the unconscious mind, failed to realize that a man's atheism can be as emotionally motivated as a theist's religious beliefs. The existence of God cannot be proved, neither can we find sufficient conclusive arguments to prove the opposite. But the arguments of an atheist should be sympathetically examined and this is what Dr Philp proceeds to do in his book. Having dealt with what appears to be the emotional basis of Freud's unrepentant atheism he carefully scrutinizes the evidence and conclusions of the great analyst as stated in *Totem and Taboo, The Future of an Illusion* and *Moses and Monotheism*. Freud's attack on religion has long been thought to be as illogical as it is emotional, and because of its prejudicial origin it is inevitably distorted. Dr Philp submits that the distortion arose out of Freud's reaction against anti-Semitism in the first place and was exaggerated by his limited knowledge of anthropology, by his mishandling of data that was second-hand, and by his eagerness to force all religious beliefs into the categories of illusion, thereby committing the fallacy of generalizing from a few particulars. 'He attempted to demolish religion by tracing the motives on which he thought it was based in particular cases and his own atheism was open to precisely the same form of attack'. Freud has given invaluable service in helping us to discriminate between false and valid experiences, even in religion, which the author frankly acknowledges. 'The unconscious desires and wishes give rise to the process of projection and rationalization...in forming his religious beliefs man is largely under the influence of these motives and processes'. But neither Freud nor anyone else has been able to prove the non-existence of that which is claimed to be experienced whether it be an elf or God. Where he principally failed was in narrowing his interest 'until it is concentrated on what appears to fit in with psychoanalytical theory'. Dr Philp's purpose is to make clear Freud's determination to get rid of Christianity because of its anti-Semitic association, and that the reasonableness of his attack could only be sustained by the overthrow of all religion on the ground of wish-fulfilment. Nevertheless, it is not logically sound to try and rebut the position of the atheist by presenting or asserting the claims of the theist, and Dr Philp comes perilously near to committing this fallacy in pp. 80–6. He assumes the existence of God to prove that projection can substantiate the claim that 'God is primarily seeking man rather than man creating the image of

God'. However, he has done an excellent work in revealing the flimsy data on which Freud has based his unbelief. 'I believe that Freud's case against religion in general was unsound and reflected his own opinions rather than the findings of psychoanalysis'. Clearly he shows that anyone wishing to proclaim atheism would be well advised not to quote Freud.

W. E. SARGENT

Introduction to Psychopathology. By LAWRENCE I. O'KELLY and FREDERICK A. MUCKLER. London: Staples. 1956. Pp. xi+704. 50*s*. net.

This book is the second edition of one first published in America in 1948. It has been written jointly by the Professor of Psychology at Illinois University, and his Research Assistant. It is designed for advanced university students, and each chapter concludes with a list of topics for discussion and suggested reading and bibliographical sources. The book also contains a useful glossary and a long bibliography. This collection of references is, as might be expected, excellently complete and the book is useful for this alone. Some of the text, however, is rather elementary, as when the reader is warned not to confuse factor analysis with psycho-analysis.

The book is divided into four parts dealing respectively with: introduction to basic concepts, the problems of disordered behaviour, the causes of disordered behaviour, and theories and treatment of disordered behaviour.

Despite its title this book could equally have been called a text-book of abnormal psychology. It is disappointing that so little space has been given to treatment and prophylaxis and none whatever to techniques of investigation.

Nevertheless, this book will undoubtedly be useful to students, although the professional clinical psychologist still waits for a comprehensive text-book on techniques of investigation, methods of treatment and their underlying rationales. However, it is probably too early to expect this.

RALPH HETHERINGTON

Prenatal and paranatal factors in the development of childhood behavior disorders. M. E. ROGERS, A. M. LILIENFELD, and B. PASAMANICK. Baltimore: Johns Hopkins University. No date given. Pp. 157. No price given.

This monograph describes an ambitious and carefully controlled investigation into the frequency with which abnormalities of pregnancy and delivery occurred in the histories of 1151 white and non-white elementary schoolchildren in Baltimore, who were considered by their teachers to suffer from behaviour disorders.

When all the children with behaviour disorders are grouped together, a greater frequency of abnormalities in the prenatal and paranatal periods can be demonstrated among them than among a series of 902 controls. A high proportion of these disorders occurred in the children who had been born prematurely. These findings are of great interest, but from the practical point of view the study would have been of more value if the authors had attempted to ascertain which particular forms of behaviour disorder were associated with which abnormalities in the prenatal and paranatal periods. Unfortunately, the only group they were able to study in this way was the one they labelled 'Hyperactive, confused-disorganized cases' in which most workers already accept the aetiological importance of birth injury—an opinion confirmed by the writers.

As it stands, the investigation merely shows that there is an apparent association between the occurrence of behaviour disorder and a previous history of abnormal prenatal or paranatal periods in a large series of cases. No directly causal effect can be attributed to the abnormalities of the prenatal or paranatal periods on the evidence offered, and it would require much closer study and more detailed classification of the forms of deviant behaviour observed before any symptoms or group of symptoms could be considered to be due to the abnormalities of the prenatal or paranatal periods which they noted. Such a study would obviously be well worth while, but it is doubtful if it could be done without the investigators having much closer clinical contact with, and knowledge of, their patients than is evidenced in this exhaustive statistical analysis.

T. T. S. INGRAM

Suicide in London. By PETER SAINSBURY. 1955. London: Chapman, Hall. Pp. 116. 15*s*.

This ecological study of suicide develops the sociological approach pioneered by Durkheim and by workers in the U.S.A., including Cavan, Faris and Mowrer. It is a major contribution, providing a valuable body of facts and a model of scientific method, and should be read by all those interested in social psychiatric investigations.

Designed to test a carefully formulated hypothesis, two complementary investigations were undertaken: (1) a statistical correlation of the suicide rates of the London boroughs with selected indices of their social characteristics, using largely the Registrar General's publications and Smith's 1932 *New Survey of London Life and Labour*; and (2) an analysis of the records of 409 suicides reported to the North London coroner in 1936–8.

Sainsbury has shown clearly that the suicide rates of the London boroughs differ from one another, and that these differences correlate with certain social characteristics of the districts—their social isolation, mobility (whether of class, occupation or neighbourhood), and disorganization (as manifest in divorce and illegitimacy rates). These correlations, and the detailed findings of this and similar surveys, are fully and critically discussed.

No attempt is made to claim too much for the findings of the second half of the investigation. Sainsbury rightly points out that the social and statistical approach must be complemented by studies of individual cases, and that the best of coroners' reports provide only very partial information about the personal factors involved. He stresses the need for more intensive psychiatric and psychological studies of attempted suicides. A useful bibliography lists the relevant literature only to 1950.

I. R. C. BATCHELOR

Man, Motives and Money: Psychological Frontiers of Economics. By ALBERT LAUTERBACH. New York: Cornell University Press; London: Cumberlege. 1954. Pp. xiv + 366. $5.

Prof. Lauterbach, a teacher of economics, takes his stand on the belief that people in their economic roles function no differently than in their other roles; in particular, they are always complex figures whose attitudes, motives and behaviour are affected by their culture, society, personality and all the rest. This is not to admit, he goes on, that psychology can replace economics; but isolation and analysis of 'economic' behaviour can only be useful if it does not ignore the psychology of personality, of perception, of unconscious processes and of communication. Nor is it possible for economics to go its own way and then 'apply' the finished findings of psychology (as one has heard economists argue); it is the approach to the exploration of economic processes that must be psychological.

The first topic taken up is business behaviour in our society, and the factors that influence it and its underlying motivation. The range of discussion is wide, including motives of competition, price determination, industrial location and investment decisions, myths and symbolism in business life, and emotional factors in beliefs about free enterprise. Summing up, what evidence there is, is to the effect that important influences of a non-economic type 'are not an exception but a rule in determining the ways in which business is started, expanded and currently done'. The point is that these influences are cultural, social, psychological, almost anything but what might be called economic. Thus in price determination it seems that a price 'often has little relation to the costs of manufacture; sometimes it is vaguely connected with whatever is considered a fair price'—an intriguing link with the fair day's work of the Hawthorne bank memory!

The second main topic is the relation of economic instability to personal insecurities and especially the implications of prosperity without stability. It is argued that socio-economic instability affects personal insecurities so that these react again on socio-economic attitudes and behaviour. The third part of the book strikes out even more widely (one almost said wildly) into the psychological implications of economic reform. The reason why people have rarely followed prescriptions for economic improvement, the writer argues, is that these prescriptions seldom took account of the attitudinal and emotional implications. The danger is, however, that reform programmes may be put forward which, though purporting to be economically based, are simply exploiting emotional needs in the service, for instance, of political ideologies. It seems to him an important task to help people distinguish between genuine economic requirements and 'pseudo-economic rationalizations of unfulfilled emotional needs'. A major difficulty, it seems to the reviewer, is to know where one rationalization ends and another begins.

The author's basic point of view is not new. As long as fifty years ago economists were asking for a closer relation with psychology. On the other hand, those economists who are interested in the traditional theoretical formulations will at once admit that they deal with almost pure abstractions; they will add, however, that they like it that way. The book itself is uneven, and obviously it must be rather speculative, although the author is the first to stress that evidence is not available to support more than a very few of the generalizations. He paints economic psychology with a rather broader brush than does, say, Katona. But his aim is to offer challenging hypotheses and stimulating research suggestions. His belief is that only from multi-disciplinary research can their verification come. And when he argues that other social sciences (e.g., cultural anthropology) have something to say which may prevent new attacks of ethnocentrism in economic thought one cannot, as a psychologist, control a rather guilty start.

R. M. McKENZIE

How people vote. By Mark Benney, A. P. Gray and R. H. Pear. London: Routledge and Kegan Paul. 1956. Pp. xii + 227. 25*s.*

This is the final report of a study by a group of London School of Economics sociologists of electoral behaviour in the Greenwich constituency during the 1950 General Election. They used the Lazarsfeld technique of repeatedly interviewing a representative panel of electors, and analysed the way the votes went in terms of such variables as age, class and interest in politics. They discuss the stereotypes of the political parties and the attitudes of members and others to national issues.

All this is a great improvement on the usual agglomeration of electoral gossip (plus the cube law) which normally appears in this field—although it must be said that about one-third of the book is taken up with just such detail. But it is in the remaining two-thirds that the 'meat' of the work lies.

From the social psychologist's viewpoint, there are two main criticisms of the material presented. The results are given in a large number of direct comparisons of percentages. (61% of the middle class, but only 44% of the working class, described themselves as being interested in politics; so did 47% of those aged 65 and over, as against 49% of those aged 21–39.) One would have liked a rather more sophisticated (and less tedious) way of dealing with the data. Even in the present format there should have been a definite statement as to the statistical significance of each difference discussed. This is so even though we would, of course, agree with the authors that it is sometimes profitable to discuss statistically non-significant differences. The second point is more important and concerns the rather slipshod way in which some of the opinion data are treated. For example, the authors seem quite unaware of the techniques for constructing an attitude scale, and the use of their 'Index of Political Opinion' is quite illegitimate. The results they obtain with it (e.g. that two-thirds of Conservatives are strongly Conservative whereas only a third of Labourites are strongly Labour) may be quite misleading, since no account is taken of the bias of their measure.

This is a difficult book to evaluate. Judged in terms of the literature of what has come to be called 'psephology', it is undoubtedly the best work to have appeared in this country so far. And here the matter might rest were it not for the high standards that the book itself sets up in a brilliant first chapter (by Benney) on the purpose and perspective of this type of study. We were promised a comparison between 'the public universe of politics and the millions of private universes of the electors', or, put in other terms, a working through from political science to social psychology. By these standards the book is disappointing. The reason for the failure lies, I think, in the fact that to attain an interdisciplinary end, a project must itself be interdisciplinary. This sort of study calls out on almost every page for the work of, say, psychologists and social anthropologists; it must be a co-operative venture if it is not to be shrugged off by the non-participating disciplines as superficial, pedestrian or misguided. At this level, this review is merely a psychologist's shrug.

DEREK PUGH

Interpretation of Sociometric Choice Status. By Åke Bjerstedt. Lund: Gleerup. 1956. Pp. 408. 40 Sw. Cr.

In recent years, sociometric techniques have been used increasingly to explore social phenomena in a great variety of situations. The now common procedure of requiring the individual to show his willingness to interact with certain individuals as well as his unwillingness to interact with certain others, has, in its simplicity, a methodological attractiveness. But all too frequently, it would seem, the surface simplicity of the method has masked its complex character. In the absence of established methods and rigorous interpretative examination of results, many sociometric studies have yielded obscure and often conflicting conclusions. What would seem necessary if sociometry is to come of age is to attempt to gain a unanimity in what sociometry studies, a scientific rigour in the methodology, and a sound theoretical framework for what is purposed by using the techniques. In the book reviewed here, a serious attempt is made to attain certain of the objectives.

The author discusses and reports tests of a series of common interpretative assumptions concerning sociometric choice status. In his task, he sets out first to discover the definition and delimitation of sociometry. His questionnaire to different authors who have written on the subject reveals much terminological confusion but also several ways in which agreement might be reached. In the first part of the book it is also necessary for him to discuss the general principles for collection and analysis of socio-preferential data and to decide on their suitability in testing the assumptions.

Parts Two and Three of the book are devoted to the testing and reporting of outcomes of the assumptions that sociometric status scores are: (*a*) relatively unaffected by preference methods; (*b*) relatively unaffected by choice aspect; (*c*) relatively constant over a period of time; (*d*) accompanied by the individual's awareness of his own relative sociometric situation; (*e*) correlated with measures of general 'adjustment' and 'efficiency' of the individual; and (*f*) amenable to similar interpretation in different countries. In Part Two an internal analysis of socio-preferential variables is made from a broad sample of Swedish schoolchildren. Attention focuses in Part Three on a smaller sample of subjects representing extreme socio-preferential positions. An extensive comparison is made of the socio-preferential variables with a series of extra-socio-preferential variables. The outcomes of the tests reveal a fair amount of non-specificity with respect to methods, choice aspects and test moments. There is evidence presented of a certain awareness by the individual of choice status received. Several clear extra-socio-preferential correlations are found with respect to specific aspects of 'adjustment' and 'efficiency'. Finally, in those areas where comparison is possible, the Swedish results show fundamental similarities with Anglo-Saxon reports.

Throughout the discussion and the two studies, there is ample evidence of a vigorous scientific inquiry. The work of most sociometric researchers is given a careful examination and the results are compared with the author's work. A continual attempt is made to effect a synthesis or to improve on the techniques. Such an approach points out many areas that require further—and often immediate—attention. Those using sociometric techniques will find many valuable suggestions in the book. For those who have not used sociometric methods and are attracted towards their use, this book should be required reading, for, as few other more general works on sociometry have done, there is illustrated here the essential 'frontier' character of sociometric research, the still unfinished state of the techniques, the dangers of over-simplification and naïve interpretation, and the tentative character of most results. The author's concluding remark is that 'there is still much to be done'. This would seem true not only for the methodology of sociometry but for all that is purposed by using the techniques.

A. R. MACKINNON

Soviet Attitudes toward Authority. By MARGARET MEAD. London: Tavistock. 1955. Pp. 148. 21*s*.

The subtitle of this book, 'An Interdisciplinary Approach to Problems of Soviet Character', indicates its aim, which is to trace the impact of Bolshevik ideas and policies on the older Russian character and its consequent transformation. The account of traditional Russian character structure is largely based on Gorer's exposition of the consequences of swaddling, although this is not directly mentioned. The continued persistence of some of the older traits is shown to result in contradictions and conflicts, whence arises the necessity of constant rigid control and the stress on self-criticism.

Most of the work is devoted to a discussion of the way in which these fundamental attitudes influence leadership roles and the relationship between the leaders at various levels of the hierarchy and the masses. There is much emphasis on the now odious personality cult, a fact which is understandable, as the book first appeared in America in 1951.

The chief, and inevitable, weakness of the study lies in its reliance on indirect materials, mainly pre-1950 Soviet publications and interviews with emigrants whose reliability is hard to assess. Given these limitations, the book is stimulating and certainly offers valuable insights into past official Soviet attitudes. A crucial question is implicit in a statement by the author herself: 'These materials provide no direct information on the extent to which those to whom they are addressed believe what they read or on the extent to which they act upon it.' It is possible that much of the material used represents a paper curtain which screens rather than reveals the actual attitudes and behaviour of Soviet citizens. Reports of reactions to the Khrushchev speech at the XXth Congress tend to point in this direction. However, until it becomes practicable to investigate attitudes directly, judgement will have to be suspended.

G. JAHODA

Papers of the Michigan Academy of Science, Arts, and Letters. Vol. XLI. Michigan: University Press; London: Cumberlege. 1956. Pp. viii+368. 80*s*.

Only the last four papers of this volume are of direct interest to psychologists. One paper concerns a longitudinal study of factors likely to predict children's readiness for school. Another treats style of emotional expression as related to social class, suggesting that lower-class expression is more immediate and bodily and less indirect and symbolic than middle-class, and that this is reflected in the forms of psychopathology to which members of different social strata are prone. The third reports techniques of rating the ambiguity or structuredness of therapeutic situations. The last investigates cognitive reorganization following electroconvulsive treatment and concludes that, during recovery, orientation returns first to personal identity, then to place and other people, and last to time. The papers' brevity precludes their giving more than a hint of these four research programmes being conducted at Michigan, and it would be extravagant of any library to buy the volume only for their sake. It is to be hoped that fuller reports of these programmes will soon be available in a form more suited to the interests and purses of psychologist readers.

I. M. L. HUNTER

Horse Psychology. By MOYRA WILLIAMS. London: Methuen. 1956. Pp. xvi+194. 16*s*.

This book comes of letting a clinical psychologist loose in the paddock. The expected result might be a psychology of the centaur, but Mrs Williams succeeds in animalizing her beloved subjects with commendable detachment. Her explanations of horse behaviour are eclectic, leaning rather heavily on animal ethology. The horse has been used in so few experiments that most of the evidence quoted comes from other animals; however, some ingenious pilot experiments on her own horses are described and wittily illustrated. The book is ideal for adult education classes, and many psychologists will read it with pleasure and profit. Perhaps Mrs Williams might reach an even wider audience if an erratum slip were inserted: for 'horse' read 'child'. As it is, it will surely be placed in many Christmas and birthday lists.

BRIAN FOSS

Medizinische Psychologie. (11th edition revised.) By E. KRETSCHMER. Stuttgart: Thieme. 1956. Pp. viii+382. DM. 29.60.

Most text-books of psychiatry bear the author's stamp to a degree quite unknown in science or in the better established medical specialties. Kretschmer's views have always been particularly strongly held, and this book is very much an expression of his own views and a presentation of his own system. No account is taken of criticisms, no mention is made of empirical work disagreeing with Kretschmer's own views. In the psychiatric field, these views are usually interesting, stimulating and derived from close clinical experience. As suggestions for research, they will be of interest to the psychologist. As a text-book, however, the work is too one-sided to be recommended.

To the psychologist, the last few chapters dealing with personality tests, projective tests and psychotherapy, are probably the most interesting. Unfortunately, they are well below the standard of the rest of the book. Kretschmer is clearly covering a vast territory without any specialized knowledge of the literature or even of the principles according to which validity and reliability are to be judged. It is perhaps characteristic that hardly any references are given and that those which are given are either out of date or refer to Kretschmer's own work. Hardly any of them are in English.

Kretschmer, in his introduction, considers his treatment of psychological tests to be rigorous, logically structured and orientated towards essentials. It is difficult to find any evidence for this belief in the chapters themselves.

H. J. EYSENCK

Das Gemüt. By S. STRASSER. Utrecht/Antwerpen: Het spectrum; Freiburg: Herder. 1956. Pp. xix+291. No price given.

Though the philosophical analysis of emotion, feeling, affect and related phenomena has received considerable attention, there appears to be no systematic phenomenological survey to offset the increasing weight of the 'positive sciences' in this field. It is the author's aim to present such a doctrine, using phenomenological evidence as a basis for a holistic-structural and non-causal interpretation.

A wide area of emotional and feeling tone is covered. Although relying extensively on traditional, particularly scholastic and modern (Hartmann, Scheler) philosophy, it is heartening to see to what extent outspoken psychological theories (Krüger, MacDougall, Sander, Stern) are preferred to existentialistic ones (Merleaū-Ponty, Sartre). That is, unfortunately, where this certainly stimulating and painstaking treatise rests. Instead of making the most needed attempt towards reconciliation between philosophical psychology and empirical evidence, the latter is too easily dismissed, using only a very insufficient and unsystematic reference to experimental and theoretical literature.

J. C. BRENGELMANN

Der Aufzähl-Test (*The Naming Test*). By ADOLF BUSEMANN. 1955. München/Basel: Reinhardt. Pp. 174. No price given.

The subtitle is 'Investigations into the World of Experience of Healthy and Neurotic Children'. The world here means 'things': the realm of object relationships. This, in the author's opinion, throws light on the child's relationship to the world in the wider sense, including personal relationships.

The instruction requires the naming of 'visible' objects for 10 min. Abstract and invisible things are not scored, nor are repetitions of the same word. Scoring is based on the total number of words, but attention is given to a number of features such as the distribution of the words within the 10 min.; the 'profile' of the graph produced; length of the time elapsing between instruction and the first word, and between words or groups of words; number of repetitions of the same word; groups and classifications according to subject or complex; the number of things present and not present, etc.

Results show that records are fairly constant for the same individual, but are sensitive to changes in mental state and to organic changes, e.g. brain injuries.

As a frame of reference for interpretation, the author gives a detailed and comprehensive description of personality assessment, remarkable for a well-balanced attitude to the different schools of thought, and for honesty about the possibilities and limitations of the test.

This test seems a very promising diagnostic instrument, well worth developing. It seems to yield valuable information at the least expense in time and effort. It touches on the deeper layers of the personality and lends itself to interpretation in terms of dynamic psychology, without disturbing the subject.

The numbers on which the present investigations are based are too low to draw conclusions as to the validity of the test. It is to be hoped that the author will develop the pattern of scoring, so as to provide an instrument for the clinical psychologist.

MARTHA VIDOR

Genie, Irrsinn und Ruhm. By WILHELM LANGE-EICHBAUM. Basel: Reinhardt. 1956. Pp. 628. Swiss Fr. 36.70.

This is the fourth edition of a classic which was originally published in 1927. In the second and third editions, the main text had been left unchanged and a large number of additions, footnotes and postscripts were added. The author had hoped himself to rewrite the work completely, but was prevented by his recent death from doing so. W. Kurth undertook the task and has carried it out in a manner which would appear to have been in line with the original author's intentions. Some sections have been eliminated altogether, the list of references has been brought up to date and considerably enlarged, and all the previous addenda have been incorporated in the text. The result is a very much bigger book, which, none-the-less, has retained its readability and its general organization. It has also retained its stress on the sociological conception of genius.

It would not be reasonable in a brief review like this to summarize or criticize a classic which ought to be familiar to all psychologists. If it were not for the language barrier, the interest of the topic and the high quality of the style would together have ensured a much wider knowledge of the main thesis of this book. It is to be hoped that this new edition will be made available to English-speaking readers, although the fact that hardly any of Lange-Eichbaum's references are to the English literature may speak against this.

H. J. EYSENCK

OTHER PUBLICATIONS RECEIVED

Acculturation: Critical Abstracts, North America. By B. J. Siegel (Ed.). Stanford: University Press; London: Cumberlege. 1956. Pp. 231. 32*s.*

Therapeutische Psychologie ihr Weg durch die Psychoanalyse. By W. M. Kranefeldt. Berlin: de Gruyter. 1956. Pp. 152. DM. 2.40.

A Handbook of Medical Hypnosis. By G. Ambrose and G. Newbold. London: Baillière, Tindall and Cox. Pp. 256. 21*s.*

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